Richard L. Garwin
Cold Fusion Panel
June 22, 1989

Please do not circulate. Will. RLGarwin

Will Happer's comments on the visit to the University of Utah with the ERAB Cold Fusion Panel on June 2, 1989

I. The Discussions with Dr. Stanley Pons

Basic measurements. The PC computer we saw was programmed to record two measurements:

1) The voltage drop across the electrodes, from which one can infer the electrical power deposited in the cell.

2) The resistance of a thermistor, from which one can infer the cell temperature, and from a proper calibration of the system, the heat flow to the surroundings.

Were the calorimeters really calibrated? The most confidence-shaking part of the Pons work was the calibration, or lack thereof, of the calorimeter. When we visited the laboratory, we saw two baths, filled with circulating water and thermostatted at about 30 C. Each bath was designed to hold four or more electrolytic cells. The cells were supplied with three pairs of electrical leads. One pair supplied the constant electrolysis current. By measuring the potential drop across this pair and multiplying by the constant current value it was possible to determine the electrical power input to the cell. A second pair of wires was used read the thermistor resistance. From supposedly well known calibration curves one could infer the cell temperature from the resistance reading. A third pair of leads could be used to supply current to the calibrating heaters. The heaters were a string of fairly high-power carbon(?) resistors connected in series within a J-shaped glass tube to keep them dry. The J-shaped tube was inserted to nearly the bottom of the electrolytic cell. I noticed that only one of the cells actually had leads attached to the calibrating resistors. The leads to which no wires were attached seemed never to have had any wires attached at all. They were straight, unscratched and unbent. *My impression is that only one of the cells could ever have been calibrated.* However, a laboratory computer was recording the voltage drops, at constant current, between the anode and cathode and the thermistor resistances from each of the cells once every five minutes. The readings were stored on diskettes I believe.

Do calibration curves exist? After leaving the laboratory, we questioned Stan on the details of his experiment and particularly on the calibration. Stan's description of the calibration was vague and confusing, at least to me. It appeared that he was doing periodic checks of the *differential* calibration of his cells. From time to time he would apply a few watts of thermal power to the cell by passing current through the calibrating resistors. Of course this calibrating power could be measured very precisely. However, the calibrating power was in addition to a substantial amount of power already dissipated in the cell by the electrolysis current. This procedure did not seem to answer the basic question: given a measured temperature difference DT between the cell and the bath, how much heat is flowing out of the cell? Stan seemed to assume that dQ/dt = C DT where C was some constant which was independent of DT. Of course, this is "Newton's law of cooling." However, it is well known that heat flow can be a more complicated function of DT and only for small DT can one be sure that Newton's law of cooling is valid. The two most important deviations to Newton's law of cooling are from convection, for example of the air between the outer wall of the cell and the inner wall of the dewar, and thermal radiation across the vacuum space between the dewar walls. I would expect convective processes in the air and also in the heavy water to be most important.

As many people have already noted, the cells were not stirred. This means that the heat transfer depends critically on the mechanisms which counter the natural tendency of a heated fluid to stratify, with the hot fluid floating on top of the cooler fluid. Although the Utah people stoutly maintain that the bubbles are adequate to thoroughly stir the solution, other groups with similar cells report that stirring makes a substantial difference in the results. Of course, the resistive heater does not produce bubbles at all, so the Pons-Fleishman arguments about bubbles as a good stirring mechanism are questionable for the calibrating heaters. Perhaps this is why the calibrating heaters were always used while electrolytic current was going through the cell and presumably producing bubbles. It is not clear how one could do an absolute calibration with the heating resistors without some mixing mechanism which mocks up the mixing of the bubbles.

If I correctly understood the talk of the Utah metallurgists (about whom, more below) they tried to calibrate the heat flow measurements in their cells by electrolyzing ordinary water, although I was so distracted by the puzzling "excursion," that I did not get the details of their procedure.

After repeated questioning, Stan showed us his "black box," which was a viewgraph with assumed heat flow mechanisms, but he was never able to show us a calibration curve for any cell. Did a calibration curve exist at the time of our visit? The metallurgists did have a calibration curve which they readily found for us.

Dewars. For still unclear reasons, the cells which Stan showed us were contained in vacuum jacketed dewars, made in the local glass shop and evacuated and sealed on the premises. The dewars were not silvered inside so radiative heat flow across the "vacuum" gap would have been relatively high. I also have some doubts about whether the vacuum was hard enough to prevent heat transport via residual water vapor which slowly outgasses from most glass systems. There was no sign of a getter inside of the dewar. This question about the efficacy of the dewar is especially important if, as I suspect, the cells were not individually calibrated with resistive heating, and a common calibration factor was used for all of them. If I understood correctly, "higher-power" cells were not contained in dewars. I guess the dewar was used to increase the temperature rise associated with the smaller heat flow in low-power cells. However, the exact path of the heat flow is unclear. If the dewar was really insulating well, it could well be that heat was carried by convection to the top of the cell and at least partially released to the room air, which was not as well thermostatted as the water bath.

Lack of crisp answers to questions. Time and again we received fuzzy if not evasive answers to questions. Although our group was billed as an important set of visitors, and although Allen Bard had previously explained to Stan Pons that we wanted to work through the nitty-gritty details of how one analyzed the data from a cell which was generating excess heat, no cell was operating when we arrived because of a power failure. Key pieces of data, especially that pertaining to the calibration of the calorimeter were missing.

Did we see a Potemkin Village? Our visit to Stan's laboratory left me with the feeling that we had seen the classic Potemkin village. We did not see a working cell. We did not verify the crucial calibration procedure, but instead we got "trust me" as an answer. Most of the cells seemed never to have been calibrated at all, and they also seemed to have been hurriedly thrown together to impress credulous visitors rather than to take data.

II. The Discussions with the Metallurgists

According to Vice President Brophy, a group of metallurgists, led by Dean Wadsworth, from the University of Utah had independently confirmed the generation of excess heat, most spectacularly in an "excursion."

I had a better impression of the metallurgists than I did of Stan and his people. For one thing, they immediately produced a calibration curve when we asked for one. The measured function of heat flow out versus temperature rise in the cell was not at all linear, as Pons assumed, but I had the feeling that the nonlinearity was in the wrong direction. That is, it seemed that the thermal resistance increased for larger temperature differentials rather than decreased, as one might expect from convective or radiative processes.

The "Excursion." The data on the excursion consisted of a viewgraph of the voltage at constant current across the cell electrodes and also the cell temperature as a function of time. The total elapsed time was on the order of half a day. Some time during the night there was a sudden rise in temperature of about 20 C. The cell was unattended and the experiment was terminated the next morning when someone came in to refill the cell with heavy water. As I pointed out at the time, there was almost no change (a few per cent at most) in the voltage (about 10 volts) across the cell electrodes during the excursion. Since a temperature rise of 20 C would have lowered the resistance of the electrolyte by at least some tens of percent, the cell voltage data and the temperature data are completely inconsistent, and I think the excursion must be discounted as some sort of experimental problem, for example, a loose connection in the thermocouple power supply.

[illegible] → 2 Thermocouples
→ 2 Counters

III. The Discussions with Dr. Michael Salaman

Dr. Michael Salaman gave us a very clear description of his search for gamma rays or neutrons from the Pons cells. Contrary to earlier assertions by Pons and Fleishman, there are no neutrons or gammas above the natural background. The gammas were examined with a large sodium iodide scintillator, located directly beneath one of Pons's water baths. Very low limits could be set on the 2.2 Mev capture radiation of neutrons in water and on the 24 Mev radiative capture radiation of two deuterons into the ground state of helium-4. The neutrons were detected by attaching a uranium-235 foil to a plastic (lexan?) substrate and subsequently etching out the damage track of the fission fragments. Detection sandwiches were placed under each of the cells, including cells which supposedly were and were not producing excess heat. The experiments seemed very solid to me.

IV. What did we learn?

In my view it is irresponsible to say to the press that we did not "find a smoking gun." Someone in our committee has already made that statement to William Broad of the New York Times, and Broad told me today, June 6, that he was about to run a page-1 story in the Times, "Blue-Ribbon DOE Committee Endorses Cold Fusion" or some such interest catcher. I begged him not to do it, but I refused to discuss the details of what I saw. What really happened was the following:

1) There was no cold fusion in Utah on June 2 because of a "power failure" just before our visit. Once again we must accept "trust me." Sure, but let me cut the deck.

2) The purported calibration procedure of the heat flow is highly suspect.

3) Most of the cells probably were not calibrated at all.

4) The purported "excursion" in the Metallurgy Department was internally inconsistent.

5) Contrary to earlier assertions by Pons and Fleishman, there is not a trace of gamma or neutron signals near their apparatus. (Trust me, remember!)

I would be happy to endorse cold fusion if I were given the opportunity to see a working cell and verify the calibration of the heat flow measurements. I think this might take about a week of my participation in the experiments. Should the excess heat really be present and due to nuclear fusion (that is, not of chemical origin) I volunteer to join my committee members in carrying Pons in a sedan chair to Stockholm for his Nobel Prize, and I would rejoice to see him become a trillionaire for a discovery of such benefit to mankind. On the other hand, I object to whitewashing a religious cult under the aegis of ERAB.

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9 JUN 89 13. 40
-R.L. GARWIN-

June 7, 1989

TO: Cold Fusion Panel

Subject: Los Alamos and University of Utah Site Visits

Enclosed for your information are the trip reports from Los Alamos and University of Utah.

Bill
William L. Woodard
Panel Secretary
(202) 586-5767

Enclosures

Cold Fusion May 23-25, 1989 Workshop,

May 24, 1989 ERAB Subpanel Meeting and May 25, 1989 Los Alamos Trip

Not surprisingly, both the numerous negative and the positive results were subject to challenge.

Texas A&M reported:

(1) Heat similar to the University of Utah results (26 watts per milliliter of palladium) and the heat appears to be related to the lithium concentration,
(2) Neutrons up to 4 times background, and
(3) Tritium increases of up to 10,000-fold and these concentrations were confirmed by Los Alamos. Privately, PNL stated they had also confirmed these concentrations.

Sweden reported neutron bursts of up to 10 times background.

Brigham Young University (BYU) reported neutron bursts of up to 6 sigma and BYU agreed to have Yale/Brookhaven test a neutron producing cell. BYU noted their effect tracked with the type of lithium used and they will try using lithium-6. During the Los Alamos visit, preliminary data was shown which indicated a confirmation of the BYU results and that, unless the neutron bursts are spread out, they appear as statistically insignificant single neutrons.

Gran Sasso/BYU reported neutron bursts of up to 7 sigma.

Los Alamos/BYU reported random neutron emissions of up to 11 sigma from Frascati-type experiments tested with very sophisticated and well established detectors in use for nuclear material safeguards. Fourteen bursts were observed of up to 300 neutrons, contained in less than 100 microsecond intervals.

Frascati discussed their previously reported neutron data of up to 500 times background and bursts of neutrons of up to 35 times background. They now state that these bursts consisted of individual neutrons arriving within microseconds of each other.

IV. Electrolytic solutions (including additives and analysis)
V. Current densities
VI. Charging times
VII. Cathode loading (and analysis)

(b) Diagnostics:
 I. Neutrons and gammas (and X-rays), including detection of bursts
 II. Tritium, helium-3 and helium-4, including cathode, solution and off-gas

(c) Natural processes: (geophysics and astrophysics)

(3) Recommend general level-of-effort for basic research (in addition to considering an FY90 SBIR topic and supercomputer time)

I have already sent the information requested by individual Subpanel Members; i.e. geophysics information to Lipman, and Wolfe's tritium viewgraph to Hoffman. Huggins declined to provide, at this time, the heat viewgraph for Bard. I informed Bard of this and this will be discused with Huggins during the site visit.

Woodard has agreed to sent the weekly, express mail or fax, distributions of information the Subpanel requested: e.g.

(1). Information Garwin sent (forwarded by Woodard's May 22, 1989 memo), which included the Case Western Reserve report of 6 watts per cc excess heat and an up to 4 sigma neutron signal.
(2). The two Canadian papers on calorimeters.
(3). The four Sante Fe "Binder" papers on Utah and MIT theories, Canadian gamma measurements and Rochester neutron measurements.

Recomendations:

Request the national labs provide the following information:

A. By June 20, 1989, a brief summary for the Subpanel of the work to date, and
B. Later, efforts the labs believe are worth pursuing in the near-term (e.g., until the November 15, 1989 Subpanel report).

DOE 1325.8

United States Government

Department of Energy

memorandum

DATE: JUN 6 1989

REPLY TO ATTN OF: ER-20.1

SUBJECT: ERAB Subpanel on Cold Fusion June 2, 1989 Site Visit to the University of Utah

TO: Bill Woodard, ER-6

In attendance were Ramsey (Subpanel Co-Chairman from Harvard), Bard (Texas), Happer (Princeton), Faulkner (Illinois), Miller (Bell), Finn (ERAB Executive Secretary) and myself (Subpanel Technical Advisor). Ramsey suggested this trip report and the updated version of my May 31, 1989 trip report (attached) be sent to the Subpanel Members to provide a summary of (only) what was reported (with the Subpanel Members to, or course, provide the critical review).

About three hours was spent with Pons. After a tour of Pons' Lab, the discussion focused on a working cell with a 1.25 cm by 0.4 cm Pd cathode and 64 milliamps per cm^2. Pons reported that, between 5.6 x 10^6 and 5.9 x 10^6 seconds (65 to 68 days) into the run, the excess power increased to up to 50 times input power and from an accumulated excess energy of 2 megajoules per cc to 8 megajoules per cc (this is about 1 to 2 kilowatt hours per cc, about 100 times that available from chemical reactions). Pons reported that between 2.6 x 10^6 and 3.2 x 10^6 seconds (30 to 37 days) after restarting the cell, the excess power initially went to 6.58 watts per cc, then to 12.6 watts per cc and then to boiling for several hours before being shut down (no measurements can be taken during boiling and the cell must be constantly manned for safety reasons). The accumulated excess energy, before boiling, was also several megajoules per cc. Pons made the following statements:

1. His heat monitoring is accurate to within 1% as confirmed by his H_2O control experiment and he will provide this data. He stated this is better than his previous control of a deuterium-loaded cathode in H_2O, which ruined a heat producing cell, due to the hydrogen. The Subpanel suggested that, as a control, he also try using a heater to replace the electroysis.
2. Later this month, he will send cathodes out for He-4 analysis. He noted that he already has 7 positive He-4 results and implied that one or more of these is (are) in the range anticipated by the heat generation.
3. His tritium comes in bursts, as he said Texas A&M reported, and his tritium is only about twice background because most is lost in the off-gas.
4. His measurements indicate he has a deutrium loading of greater than 1.0.

6. He stated that he does not provide specs to his Pd supplier and he uses the Pd, as is, from the supplier, (who reportedly uses a proprietary method to only cast, forge and draw the Pd.) He stated the one cc Pd cube, which vaporized, may have been a crystal, the Pd sheets he used next gave only marginal results and the first batch of Pd rods gave better results than the second batch he is now using. He (and the other Utah group) noted the importance of having little or no hydrogen in the rod.
7. He stated he does not use additives in the solution, he does not pre-electrolyse, he will try LiSO4 and Li-6 and the other Utah group will try NaOH.
8. He stated the scale of many of the other experiments is too small to detect the temperature changes and that these changes are too small for isothermal calorimetry.

Privately, the Subpanel noted no major errors were found and, although the calibration needs improvement, the heat bursts do not appear due to calibration.

Prior to a tour of the independent, metallurgy group lab, they provided some of the May 1989, data on 6 heat bursts of 20 to 100% excess energy without subtracting the electrolysis energy, using 4mm by 10cm Pd rods, 100 to 120 milliamps per cm^2, after 18 to 19 day runs:

1. Power in: 9.7 watts, power out: 18 watts for 24 hours, 775,000 excess joules, 58 times the energy available from burning all the deuterium in the rod.
2. Power in: 9.7 watts, power out: 58 watts for 91 minutes before an accidental shutdown.
3. Power in: 9 watts, power out: 58 watts for 40 minutes, 7 times the energy available from burning all the deutrium in the rod. After a small explosion, the power out went to 18 watts.
4. Power in: 6.3 watts, power out: 8 watts.

This group also stated:

(1). They will do a control using a heat producing, deuterium-loaded rod in H_2O.
(2). They have sent out samples for tritium analysis.
(3). They more worked rods produced only marginal heat.
(4). Analysis of the cathode surface showed Pt, Silicates, Zn and Cu.
(5). Analysis of the solution did not show H_2O_2 or "other chemicals."
(6). They will use a doppler-shifted laser to crosscheck the thorough mixing indicated by their temperature measurements at different locations and at different input powers.
(7). They will perform experiments with internal and external recombiners.

The Subpanel expressed concern that the voltage did not always drop with the temperature bursts (a second temperature monitor is indicated).

The independent physics group reported the following on Pons heat producing cells.

1. Neutron flux does not exceed 1 per second over background based on a three day run using U-235 foil and lexan polycarbonate sheets to produce tracks, with half the system shielded by Cd plates.
2. Gamma flux, including the 24 MeV gamma, does not exceed 100 per second over background using NaI on the cell which boiled for several hours.
3. Will use pulse shape discrimination and x-ray spectroscopy (with help from Los Alamos).

KTVX reported favorably on the interviews with Ramsey and with Pons and noted the $1/2 million from the state of Utah will be used up by the end of the year.

David Goodwin
David Goodwin
Special Assistant
Associate Director for
High Energy and Nuclear Physics

Attachment

cc: Professor Norman Ramsey
Department of Physics
Harvard University
Cambridge, MA 02138

W. Hess, ER-20
P. Stone, ER-1
T. Finn, ER-55
R. Gajewski, ER-16
D. Crandall, ER-54
L. Iannello, ER-11
D. Barney, ER-16
R. Marley, ER-1
W. Warnick, ER-32

R. L. Garwin
07/09/89 ✓

Will Happer's comments on his visit to Stanford University with the ERAB Cold Fusion Panel on July 6, 1989

Discussions with Professor Huggins. Professor Huggins is a very smooth talker, and (if one were to ignore the disagreements between Professor Huggins and his students and post docs on what actually went on the the laboratory) gave a more persuasive viewgraph show than either the University of Utah people or the Texas A&M people, the other two sites I visited. Professor Huggins had the least impressive laboratory of any I have seen, and I am sorry to report that we did not inspect any of the original experiments, but only bits and pieces of apparatus which were claimed to have been used in the original experiments.

We spent most of the morning looking at viewgraphs. In contrast to other cold fusion proponents I heard during site visits, who perhaps understood more than they were able to get across to the panel, Professor Huggins gave a clear discussion of the phase diagram of the palladium-hydrogen (palladium-deuterium) system and of the irreversible thermodynamics of the electrolysis of water.

Calorimetry. The discussion fell apart when we began to review what was actually done in the experiments. Once again, the focus was on calorimeter calibration. Professor Huggins showed impressive-looking calibration data which were said to represent the temperature difference between the electrolyte in his cell and the temperature of the surrounding bath. Since there was no insulation between the cells and the bath, the thermal resistance was that of a few mm of Pyrex glass in the wall of the electrolytic cell. The temperature differences never exceeded ten degrees Celsius, and most of the data was accumulated at temperature differences of four or five degrees. The "excess power" usually corresponded to temperature differences of less than one degree.

Professor Huggins told us repeatedly that the cells were calibrated by depositing a precisely measured amount of thermal power from electrical heaters into the electrolyte and measuring the corresponding rise in the electrolyte temperature, relative to the bath. From any measured temperature rise one could therefore infer a heat flow rate to the bath. There were some arguments between Professor Huggins and his students about how long one waited before assuming that the cell temperature had reached equilibrium after the energy input had been changed. If we got an unambiguous resolution of the argument, I must have missed it, but the students seemed to think that the waiting time was about thirty minutes. I was amazed at the linearity of the curve of temperature rise versus heater power in. It appeared to have been drawn by a ruler. The viewgraphs presented by Professor Huggins seemed to imply that during electrolysis, one simply waited for the temperature to settle down at some electrolytic power input to the cell, and then used the measured temperature difference to infer the heat flow to the bath from the previous calibration. The students contradicted Professor Huggins and claimed that the actual procedure was to wait for the temperature to settle down while constant electrolytic power was going into the cell, then add various amounts of electrical heater power, say 2 watts, 4 watts, 6 watts etc. and to use this real-time calibration to infer the heat flow out. This procedure has some similarities to what Professor Pons told us he did. It does not answer the question of how linear the temperature rise versus heat evolution rate really is.

Sporadicity. Professor Huggins told us that his electrolytic cells always behaved normally when they were first turned on, that is the heat output plus the enthalpy of formation of the gases was equal to the electrical power input to the cell. After some days the heat output of the heavy water cells was said to gradually rise until it exceeded the electrolytic power input to the cells, even if the enthalpy of the evolved deuterium and hydrogen gases were neglected. A viewgraph (labeled "3.Time Dependence") showing the time dependence of this excess power in a heavy-water cell had no units on either axis. When I asked about it, I was told that the horizontal axis covered several days of time. It seems impossible to reconcile the monotonic increase of the "excess power" with a data on another viewgraph, which was not included in our packet of hard copies. This last-minute viewgraph showed a very sporadic time dependence of the heat output, which varied by about an order or magnitude from day to day. In fact, there were two large "peaks" in the heat output, spaced about one week apart. The sampling interval was about one day. As Dick Garwin pointed out to Professor Huggins during the meeting, there is a serious discrepancy between the viewgraphs of our handout packet and the claim stressed in our meeting that the effect is strongly sporadic.

Constant voltage operation. Professor Huggins told us that his cells were operated at constant voltage. In contrast, almost all of the experiments at the University of Utah and at Texas A&M, were done with constant current through the cells. In principle, constant voltage operation is all right, provided that the current flowing across the cell was carefully measured. However, it is certainly less convenient to measure the current in a circuit than the voltage across a circuit element, since a current meter of negligible resistance must be inserted into the circuit. The large differences between the electrolyte resistances for light water and heavy water imply that the deuterium and hydrogen gas evolution rates in otherwise identical cells with heavy water and light water would have differed by about a factor of two. Since Professor Huggins makes much of the differences he observes between heavy water and light, it seems strange to build in this difference in electrolysis rate by operating at constant voltage.

Laboratory visit. We spent about twenty minutes in a tiny, practically empty laboratory in which new experiments were said to be planned. We were shown two sample cells which were electrolyzing water, but I gather were not part of an experiment since they were not in a thermal bath. Both of the cells shown to us were isolated from the atmosphere by small bubblers which contained about an inch of a liquid which looked like diffusion pump oil. One cell had a platinum-metal-gauze catalytic recombiner for the evolved oxygen and hydrogen (or deuterium) gas. The gases from the other cell were not recombined. About one ml per minute of gas was bubbling out of the sell without the catalyst, and no gas was escaping from the cell with the catalyst. Professor Huggins says that his group has never bothered to carefully measure the volume of the evolved gases to get a cross check on whether there is any recombination within the cells without catalysts.

pt black on teflon

After some ten minutes, one of the students brought in a thermostated shaker bath, designed for biological cell cultures I believe. This bath was said to have been used in the original experiments, but I thought I remembered seeing photographs of the cells in a picnic hamper. The electrolytic cells were said to have been completely immersed in the constant temperature bath. In contrast, the cells used at the University of Utah and at Texas A & M had their tops exposed to the laboratory air, which was not kept at constant temperature. If true, this total immersion eliminates one of the objections we have had to the calorimeter experiments elsewhere. The tops of the cells were closed with black rubber stoppers, penetrated by the necessary electrical leads for the calibrating heaters and the electrolysis, and also by a glass tube with a sealed-off bottom to serve as a thermocouple well. The rubber stopper and the leads were covered with paraffin wax to prevent bath water from leaking into the cell. The cells were not stirred, but Professor Huggins claimed that the bubbling and the shaker bath were enough to get rid of thermal gradients in the electrolyte.

All temperature measurements were said to have been made with a single thermocouple. They used an electronically compensated thermocouple rather than an ice water bath or some other thermostat for the cold junction. I believe the thermocouple was copper-constantan. Because different "compensated" thermocouples gave different readings, they made all measurements with a single thermocouple, which they moved from place to place. The thermocouple shown to us was held in a white ceramic, double-hole tube, about 2 inches in length and was inserted into a Pyrex glass well, perhaps 4 mm in diameter which was immersed in the electrolyte through a hole in the black rubber stopper. The ceramic was soaking wet because the glass well had water in it to serve as a heat transfer medium.

What did we learn?

1. We saw no operating cells, and we did not see the original laboratory. As an experimentalist, I cannot judge the credibility of an experiment without seeing it in operation.

3. The "sporadicity" reported by Professor Huggins is inconsistent with his other viewgraphs, which show a smooth, monotonic increase of the heat output over a period of about 80 hours in a heavy water cell.

4. Professor Huggins and his students do not agree on how the experiments were done. For this reason we are still not sure how the heat output was measured or how believable the claims for "excess power" are.

5. Professor Huggins is working on a phenomenon which, if real, is as portentious to humankind as the discovery of fire (to steal a phrase). Thus, I was astounded at the casualness of the Stanford group. Obvious tests like the measurement the volume of the evolved gases, were not done, nor were the painstaking searches for systematic errors which characterize a good experiment. I could not help comparing the behavior of these prophets of cold fusion with the manic, 24-hour-a-day vigils of the high-temperature superconductor pioneers. Roentgen was not seen for six months while he worked day and night in his laboratory after his discovery of x rays, before telling even his wife. Instead, we have cheerfully credulous press releases.

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Date: JUNE 20, 1989

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Number of Pages to Follow: 5

Will Happer's comments on his visit to
Texas A&M University
with the ERAB Cold Fusion Panel
on June 19, 1989

Inspection of the Bockris laboratory. I was in the first group to visit Bockris's cold fusion laboratory. We were shown two experiments, the experiments on tritium production and the experiments on excess heat production.

The tritium-producing cells. There were close to 20 cells which were said to produce tritium. The cells were on the order of 20 ml in volume and were similar to large centrifuge cells. The cells were closed at the top with a rubber membrane, similar to the type used to cover vials of antibiotics for loading hypodermic syringes. Heavy water was added to the cells with a hypodermic syringe which was poked through the membrane before injection of the heavy water. The syringes were said to be used only once and then discarded. The rubber membrane was penetrated by the two wires used to supply the current for electrolysis and by a hypodermic needle attached to transparent plastic tubing, about 8 mm in diameter. The plastic tubing led to a beaker containing mineral oil through which the gaseous mixture of deuterium and oxygen together with heavy-water vapor bubbled up to the surrounding atmosphere at a rate of a few ml per minute. The escaping gases carried with them some heavy water and black oxides of nickel or other dirt which were entrained in the escaping gas stream because of the frothing of the electrolyte. A good deal of electrolyte and nickel oxide or other dirt had accumulated in the beaker beneath the mineral oil. I would guess that the beaker contained a few hundred ml of dirty electrolyte and a few hundred ml of oil. John Schiffer pointed out that there were two bottles of heavy water from different suppliers on the table next to the hood that contained the cells. We were assured that the origin of the heavy water was well known and recorded for each addition. None of the cells was being monitored for heat release, and most were operating in the air although several, which were operating with higher currents, were in a water bath to provide more effective cooling.

Most of the cells contained nickel anodes, which Bockris says are essential for the production of tritium. The cells had been operating for various periods of time, from days to weeks. Cells which had been operating for more than a few days were filled with a black suspension which was said to be oxides of nickel.

If I understood the cell construction correctly, there was a central wire of palladium, perhaps 2 mm in diameter, which served as the

cathode. We looked at a sample of the palladium, which was from a supplier in Virginia. The anode was a nickel mesh wrapped up into a cylindrical shape and held away from the palladium cathode by red rubber spacers which could be seen at the top and bottom of the electrode assembly. I don't know about the chemical stability of rubber in the highly alkaline electrolyte of these experiments, but I would be surprised if some of the rubber and glass was not leached away during the course of the experiments. Goodness knows what was in the solutions which were added to the tritium-counting cocktail, but it seems quite likely that there were transition metal oxides and various organic decomposition products from the rubber spacers which might have been involved in slow chemiluminescent reactions.

Access to the tritium production cells. There appeared to be no control of access to the any of the Bockris experiments. They were located in a large laboratory which contained desks for many graduate students, many of whom were working away during our visit. I don't think that there is any way to be sure that the tritium producing cells were not "spiked" by someone.

Neutron activation. If tritium were really produced by d-d fusion reactions, a comparable number of neutrons would have been released if the usual branching ratios for the fusion reaction hold. If so, neutron activation products should be readily detectable in the surrounding pieces of apparatus. An example is neutron capture by ^{23}Na to yield ^{24}Na which decays with very characteristic gamma rays and is often used to determine the dose to humans who have inadvertently been exposed to neutrons. Most glass, including pyrex glass from which the cells were probably made, contains several per cent sodium oxide as a flux. Sodium activation produces a rather short-lived diagnostic gamma ray. I do not have a table of isotopes and neutron cross sections available to me so I cannot give examples, but I suspect that one could find longer-lived neutron activation products which could serve as a retroactive test for neutrons released from the tritium-generating cells. For example, there may be convenient neutron activation products from the the nickel and palladium electrodes.

The heat-producing cells. We also inspected two cells which were said to sporadically generate excess heat. These cells were much bigger than the cells used for tritium production, perhaps 5 cm in diameter and containing around 100 ml of solution. They had a styrofoam plug at the top, and this was penetrated by a pair of leads for the electrodes and a pair of leads for the calibrating heater, which was a commercial cartridge heater in a glass tube to keep it dry. The cells were big enough to contain a plastic-coated iron stirrer on the bottom, which was spinning away while being driven by two magnetic

stirring devices located below the water bath. The plug was also penetrated by commercial thermistor probes. There was a single thermistor in one cell, and there were two thermistors in the other. The electrolytic cell was placed inside a larger-diameter cell which was immersed in the thermostatic bath. The spacing between the outer jacket and the cell was on the order of 8 mm, and the electrolytic cell was loosely held in position by two rings of plastic tubing, about 3 cm apart, near the top and bottom of the electrolytic cell. Except for the plastic spacer, the volume between the electrolytic cell and the jacket was filled with air. The thermostatic bath was filled with water at about room temperature and covered with small ping-pong balls to reduce evaporation.

Thermal coupling to air. It was not clear whether the thermal coupling to the laboratory air was negligible compared to the coupling to the bath. A large area of the electrolytic cell was directly exposed to the air, and when I put my finger on the plastic plug, I noticed that it and the surrounding glass at the top of the cell were noticeably warm. I have no doubt that significant convection currents are carrying warm air out of the top of the cells, and it is not clear how much heat is flowing into the room air and how much is flowing into the bath. More importantly, however, I was very surprised to find that the cell moved freely when I touched it, about which more below.

Loose fittings. The electrolytic cells fit very loosely in the jacket, and very minor fiddling with the wires was enough to seriously tilt the cell from one side of the jacket to the other. I estimate that the motion was typically about 5 mm. We asked one of the people who normally work on the experiment to show us how he added heavy water to the cell. He used a large and frightening hypodermic syringe, and it was necessary for him to remove a rubber stopper from the top of the cell to allow him to insert the needle of the syringe. In the process, Norman Ramsey noticed that there was about a 10% change in the reading of the thermistor on the chart paper. I had earlier noticed that by shaking the wires it was possible to change the chart paper readings by around 10% in either direction, since everything was so loosely fitting. Once made, a change in the calibration was constant until someone jiggled the wires again. While inspecting the laboratory notebooks, we saw examples of both "excess heat" and less-talked-about "deficit heat," both on the order of 10% of the nominal values. Someone, Allen Bard I believe, pointed out that the "excess heat" production reported by the Bockris group can not be distinguished from the effects of changes in the calibration of the calorimeter due to jiggling of the cell within the jacket.

Visit to the Appleby-Srinivasan laboratory. I was in the second group to visit the Appleby-Srinivasan laboratory. Their work is based on

studies of electrolytic cells in a commercial microcalorimeter. They claim to observe sporadic generation of excess heat. They have never seen any tritium production, although I gather that they have looked for it. The data that they present in viewgraphs looks "raw" but in fact it is heavily massaged and supplied with plausible looking "noise." We were permitted to inspect some of the chart paper from which the viewgraphs were made, and my judgement is that the "excess heat" is in the noise. Bruce Miller, who spent more time than I did looking at the charts says that the "excess heat" generation is inferred from changes in the cell voltage. There is almost no change in the heat output.

Conducting cell walls. A serious problem with the earlier experiments was that the walls of the cell were stainless steel. Although Appleby claims that there is no net current to the stainless steel cell jacket, there is no way to estimate how much the currents carried through the conducting walls of the cell and indeed through the rest of the calorimeter, which is massive (several pounds) of aluminum blocks pressing up against the stainless steel sample cell. Cells with insulating teflon coatings are now being prepared, but I was not sure whether any had been used yet, and if so whether any of the gave excess heat.

Black-box calorimeter. It was very difficult to know what was going on in the microcalorimeter. The device was originally built to measure the heat loss attending the self discharge of commercial batteries, and it needed some modification to permit it to be loaded with electrolytic cells. Appleby et. al. do not know how the calorimeter works, but they are in the process of trying to get more information about it from the original builder. For example, they do not know where the calibrating resistors are located, they do not know how the thermopiles are arranged in the instrument or whether the thermopile voltage or current is measured. They think it is the voltage, but it it not clear from any documentation. The samples are also very well hidden inside many layers of insulation once the calorimeter is loaded, so no one can know what is going on in the cells. You can't just look inside as you can with the Bockris cells.

What did we learn?

1. The Bockris calorimeter is too loosely put together to be trusted. I don't think any excess heat was observed.
2. The Bockris tritium results are a mystery.
3. None of Bockris's colleagues has seen tritium.
4. There is no evidence for the neutrons which should have been produced along with the tritium. Someone should look for neutron activation products.

5. The controls for the tritium experiment are very poor. Many people have access to the experiment, and no one seems to be in charge. Bockris told us himself that he had many people participate in the experiments, so who knows what happened? There is a handwritten poster with instructions on how to record data over the hood in which the tritium experiment is done, presumably for the benefit of part-time helpers in the wee hours of the night. Having participated myself in a few practical jokes on senior professors (who deserved it) when I was a graduate student and post doc, I have no confidence that the experiment is secure from mischief.

6. The samples drawn out of the tritium-producing cells probably contain organic decomposition products of the rubber stopper as well as various transition metal compounds from the nickel electrode. What does this imply for chemiluminescence in the scintillator cocktail?

7. The Appleby-Srinivasan experiments are based on a poorly understood calorimeter and on signals which are very close to the noise. The stainless steel cell walls can carry current and may have shorted out, for example by frothing, some of the cell resistance, thereby making the power input to the electrolysis seem less. Since it is not possible to look inside the microcalorimeter no one could check.

Report on Visit to "Cold Fusion" Research at ORNL

Jacob Bigeleisen

16 June 1989

On my visit to ORNL on 13 June 1989 I took some time to visit with Charles D. Scott, Senior Corporate Fellow in the Chem. Tech. Division. He is heading up one of the "cold fusion" experiments at ORNL. The overall program is being coordindated by Mike Saltmarsh 574-4145. Another group is under Don Hutchinson of the Physics Division, 4-4730. Scott is measuring heat production, neutrons and tritium. He gave a report at the Santa Fe meeting. To a first approximation he sees no neutrons, no tritium and no excess heat. His cell is a glass cell containing 130 ml. of D_2O and a 40 ml. vapor volume. All the gases are vented and there is no monitoring of the amount of gas evolution. Surrounding the cell is a water jacket through which he pumps water, measures the flow rate of the water and the temperature rise in the water. The heat capacity of the water cooler is calibrated with an electrical resistor mounted insisde the cell. The entire assembly is insulated and situated behind a concrete wall to reduce the neutrn background. The electrodes are a platinum spiral and Pd rods 2.8 mm diameter by 8.5 cm. long. The electolyte is 0.2 N LiOD.

Operating Conditions:

1. current densities 100-500 ma/cm.$^{-2}$
2. Cell temperature 30.5 °C ± 0.2 °C
3. ΔT across the cooling water (outlet - inlet) 1.5 °C
4. EI input 7-8 watts
5. Heat Recovery ≈ 6 watts
6. Electrode preparation- Cast, then swaged, and then vacuum annealed at 1000 °C for 4 hours. Then held in vacuum for 2 days prior to use.
7. D_2O Now using 110 dpm/ml water from Aldrich. Previous samples had up to 72,000 dpm/ml!. Tritium counting is beind done by Jeff Wade, ORNL Analytical Chemistry Division.
8. D/Pd ratio- not measured (presumably < 1).
9. Cell operation - 4 ml. of D_2O added manually every 8 hours.
10. There is a video camera and TV screen for observing the operation of the cell.

Detailed Comments. He claims to see occasional "bursts" of neutrons. I am not convinced that they are beyond statistics. I would like to see Herman Chernoff or John Tukey analyze his neutron data.

He sees an increase in tritium, which may be beyond that calculated from electrolytic enrichment and enrichment by the evaporation of water. He does not find any extended periods of excess heat generation. He claims occasional 8 hour periods where there may be ≈ 20% excess heat. It is not reproducible, he does not consider it significant and has little confidence that this is a real effect. He showed me one of his Pd electrodes which had been used as a cathode. It was black, not shining silver. There was extensive recrystallization with large grains.

He is in contact with Dean Wadsworth of the Utah School of Mining and Metallurgy. He reports the following communicated by Wadsworth. Wadsworth finds no excess heat with pure Pd electrodes. He finds periods in which there is several times energy output over input with impure Pd electrodes.

Evaluation of the Scott setup. The heat flow measurements need further refinement in the design of the cell. There are no thermal blocks on the wires leading to the electrodes and resistors. The calibrating resistor is encased in glass, which has poor heat transfer. There are radiation losses. I estimate the overall accuracy of the measurements to be within ± 10%.

Discussion with John Googin, Senior Corporate Fellow - Y12. Googin has 35 years experience in operating large scale production electrolytic cells with H_2O, D_2O with Ni, Fe and amalgam electrodes and LiOH and NaOD electrolyte.He can make available up to 20 cells each capable of handling 2000 amperes! He explained to me how the platinum gets deposited on the Pd cathodes in all the "cold fusion" experiments. There is peroxide formed in these electrolytic cells, which is capable of oxidizing the Pt at the anode and then transferring it tot the cathode where it is reduced and deposited.

Executive Area Facsimilie
Machine
Cover Sheet

Date: JUNE 20, 1989

To: RICHARD GARWIN, STEVE KOONIN

Address/Mail Stop: JASON/MITRE

Telephone Number of Distant Copier: (619) 455 3943

From: WILLIAM HAPPER

Address: The MITRE Corporation, 7525 Colshire Drive, McLean, VA 22102

Sender's Facsimilie Number: (703) 883-5468

Comments:

TEXAS A+M
TRIP REPORT

Number of Pages to Follow: 5

Will Happer's comments on his visit to
Texas A&M University
with the ERAB Cold Fusion Panel
on June 19, 1989

Inspection of the Bockris laboratory. I was in the first group to visit Bockris's cold fusion laboratory. We were shown two experiments, the experiments on tritium production and the experiments on excess heat production.

The tritium-producing cells. There were close to 20 cells which were said to produce tritium. The cells were on the order of 20 ml in volume and were similar to large centrifuge cells. The cells were closed at the top with a rubber membrane, similar to the type used to cover vials of antibiotics for loading hypodermic syringes. Heavy water was added to the cells with a hypodermic syringe which was poked through the membrane before injection of the heavy water. The syringes were said to be used only once and then discarded. The rubber membrane was penetrated by the two wires used to supply the current for electrolysis and by a hypodermic needle attached to transparent plastic tubing, about 8 mm in diameter. The plastic tubing led to a beaker containing mineral oil through which the gaseous mixture of deuterium and oxygen together with heavy-water vapor bubbled up to the surrounding atmosphere at a rate of a few ml per minute. The escaping gases carried with them some heavy water and black oxides of nickel or other dirt which were entrained in the escaping gas stream because of the frothing of the electrolyte. A good deal of electrolyte and nickel oxide or other dirt had accumulated in the beaker beneath the mineral oil. I would guess that the beaker contained a few hundred ml of dirty electrolyte and a few hundred ml of oil. John Schiffer pointed out that there were two bottles of heavy water from different suppliers on the table next to the hood that contained the cells. We were assured that the origin of the heavy water was well known and recorded for each addition. None of the cells was being monitored for heat release, and most were operating in the air although several, which were operating with higher currents, were in a water bath to provide more effective cooling.

Most of the cells contained nickel anodes, which Bockris says are essential for the production of tritium. The cells had been operating for various periods of time, from days to weeks. Cells which had been operating for more than a few days were filled with a black suspension which was said to be oxides of nickel.

If I understood the cell construction correctly, there was a central wire of palladium, perhaps 2 mm in diameter, which served as the

cathode. We looked at a sample of the palladium, which was from a supplier in Virginia. The anode was a nickel mesh wrapped up into a cylindrical shape and held away from the palladium cathode by red rubber spacers which could be seen at the top and bottom of the electrode assembly. I don't know about the chemical stability of rubber in the highly alkaline electrolyte of these experiments, but I would be surprised if some of the rubber and glass was not leached away during the course of the experiments. Goodness knows what was in the solutions which were added to the tritium-counting cocktail, but it seems quite likely that there were transition metal oxides and various organic decomposition products from the rubber spacers which might have been involved in slow chemiluminescent reactions.

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Site Visit Report: LANL July 7, 1989 (E. Storms)

T. Kenneth Fowler 7/9/89

On July 7, I visited Ed Storms at LANL for about an hour to discuss his experiments that produced tritium. The meeting was arranged through Reed Jensen who now serves as LANL cold fusion coordinator. I was actually shown around by Charles J. ("Carl") Orth on Jensen's behalf. Orth expressed skepticism about Storms' results and noted there had not yet been a LANL peer review. Official reports from June 30 and July 7 are attached.

I visited Storms at his lab and saw the setup which is still ongoing. There were a half dozen or more cells running at the time, at 3-5 volts per cell and 0.2 amp. Each cell is a 50 ml plastic bottle with rubber cork and two insulated leads to (1) a platinum semi-circular screen anode, and (2) paladium disc cathode about like a thick penny, attached to a platinum wire. I forgot to ask about the electrolyte. The main thrust of the work has been to study D/Pd ratios vs. surface treatment of the Pd. The Pd discs are fused at LANL by Storms and coworkers from Pd powder. They find high voltages (but $\leq$ 5 volts, I think) and higher [illegible] D/Pd ratios if surfaces are treated (eg. "sulfided" in H_2S environment) - with ratios exceeding unity, up to 1.3 (volume average, measured by weighing loaded Pd disc vs. disc after driving out the D). The cells "producing" tritium were sulfided, I believe, and reached D/Pd ~ 1.0; their charging histories were recorded.

The main points I learned were:

1. Only 2 out of 51 cells have shown tritium. Storms has not been able to repeat it despite many attempts
2. The amount observed was 100 times background ($\geq$ 4000 /ml/minute; background 35 for this D_2O)
3. Contamination cannot be ruled out
   (1) Tritium was observed about 10 days into the run
   (2) There were no other measurements prior to this to give a buildup history: insofar as direct measurements are concerned, tritium could have been there all along
   (3) Shortly after detecting tritium, the polarity was reversed to make Pd the anode and "drive out any remaining tritium in the Pd" — none came out, the tritium level in D_2O did not increase (Storms interprets this as evidence of surface production but it could equally well mean no production in situ)
4. Other facts
   (1) The experiment is located in the TSTA facility where megacuries of tritium exist
   (2) Pd discs are prepared in this building
   (3) The 2 discs/cells that showed tritium were prepared at the same time (others at different times)
   (4) Jim Anderson, TSTA Manager, doubts contamination from his activities which are carefully monitored. However, per my discussion with him, we concluded that the amount in question — 10^{13} atoms in a 50ml cell showing 4000/ml/min (= 10^{-8} Curie) — is small. For example, a "swipe" showing this count rate would call for cleaning it up but would not constitute a reportable incident

Los Alamos
Los Alamos National Laboratory
Los Alamos,New Mexico 87545

memorandum

TO: Reed Jensen, ADR, MS-A114

DATE: June 30, 1989

FROM: Charles Orth, INC-11, MS-J514

MAIL STOP/TELEPHONE: J514/7-4785

SYMBOL: M331TX9

SUBJECT: REPORT ON "COLD FUSION" ACTIVITIES AT LOS ALAMOS - June 24 to June 30, 1989

An experiment to test the reproducibility of generating small neutron bursts from a pressurized D_2 gas bottle containing Titanium-662 alloy (Ti with 6% Al, 6% V and 2% Sn) at room temperature produced four short-lived bursts ranging from 30 to 100 neutrons from the source. Plans are now underway to run normal hydrogen control experiments and to use DT gas as soon as the safety aspects can be resolved.

In an earlier report it was noted that small bursts of neutrons were detected from a group of six Jones-type electrochemical cells with V, Pd or fused Pd-Ti alloy cathodes, being operated simultaneously within a neutron well counter. This week a single Pd-Ti alloy cell was run in the well counter but neutron bursts were not observed. This suggests, but does not prove, that this type of cathode was not responsible for the previously reported bursts.

A Pd cathode with a sulfided surface was prepared in the same manner as the two cathodes that appeared to previously produce tritium, and it was electrolyzed while in a neutron well counter. Neither significant tritium buildup nor excess neutrons was observed. However, a thin deposit of lead and copper from dissolution of a solder joint at the top of the cell was later found on the surface of this cathode. The cathode leads and soldered connectors of the two cells in which tritium buildup was reported were coated with a Torr Seal epoxy protective coating. Three additional sulfided electrodes with Torr Seal coatings are currently being studied. One of these is being electrolyzed in a neutron well counter in the low-background neutron counting laboratory. All three are being monitored for tritium buildup. If a significant increase in tritium is observed in the neutron counting laboratory experiment, the cell will be removed from the well counter and then be measured with a sensitive x-ray counter. If there is a significant increase in the tritium concentration, copious numbers of 21-keV Pd K x-rays should be produced via interaction of protons from the ^{3}T-p fusion channel with the Pd atoms.

In materials studies, Pd single crystals have shown a reluctance to charge to D/Pd ratios greater than about 0.7 and then to discharge back to D/Pd ratios less than about 0.6. In another experiment a Pd-Rh-Li alloy cathode has been charged to D/Pd = 1.29 and then placed in liquid nitrogen awaiting neutron diffraction studies.

A calorimeter with oxygen-deuterium gas recombination within the electrolysis cell has been constructed and a measurement with a 1 mm x 10 cm vacuum annealed Pd cathode is in progress. The calorimeter has a heat output over input sensitivity of about 1%. Resistivity of the cathode is constantly monitored to measure the degree of D loading. Neutron and gamma-ray emission from the cell are also being continuously monitored with a NE-213 scintillation detector using pulse shape discrimination to distinguish between the two types of nuclear radiation. Tritium will be assayed in the input and final electrolyte solutions.

Some theoretical work is being directed to the possibility of resonance-enhanced deuteron Coulomb barrier penetration.

There is nothing further or new to report on the following activities:

- Confirmation of experimental results claimed by others.
- Chemical assays of chemical reactants and products.
- Control experiments with light water.
- Determination of mechanisms and rates for fusion reactions in solids.
- Verification of neutron and/or heat release observed in the University of Utah experiments.

CJO:cel

cy: INC-11 File

Los Alamos
Los Alamos National Laboratory
Los Alamos,New Mexico 87545

memorandum

TO: Reed J. Jensen, ADR/A114
DATE: July 7, 1989
FROM: Charles Orth, INC-11
MAIL STOP/TELEPHONE: J514 / 7-4785
SYMBOL: ADR:89-292
SUBJECT: **REPORT ON "COLD FUSION" ACTIVITIES AT LOS ALAMOS— July 1 to July 7, 1989**

The three sulfide-coated Pd cathodes with Torr Seal protected leads and solder joints described in the last progress report have been electrolyzed for a week. The current densities in two of these were raised from 40 to 200 mA/cm^2 during this time interval, and thus far, no significant tritium buildup has been observed. The third unit is being electrolyzed at 40 mA/cm^2 while in a neutron well counter at the low-background laboratory. Small aliquots of electrolyte have been removed for tritium assay, yet to be done, and only background neutron rates have been observed.

In solid-state, D_2-gas experiments, eight Ti cathodes from Jones-type electrolysis cells were cycled together several times between liquid nitrogen and room temperature under 40 atmospheres of deuterium before a single burst of about 100 source neutrons was detected. The burst occurred during a warmup at about −30°C. Experience gained from work on the gas-metal systems indicates that 2 to 4 days of "incubation" under 40 atmospheres D_2 pressure (room temperature) is an essential step toward obtaining the previously observed neutron bursts. Although Ti-662 alloy appears to yield reproducible neutron bursts in high-pressure systems (40 atmospheres D_2), a recent experiment to determine low-pressure (3 atmospheres) behavior failed to yield neutrons. A second low-pressure experiment with pre-conditioning of the Ti-662 surface will be performed.

The effects of carbon, sulfur, and trace metal impurities on deuterium takeup in Pd are being explored in a systematic manner. Measurements in progress indicate that minor impurities have a profound effect.

The calorimeter experiment described in last week's report has been "charging" for one week at a current density of 60 mA/cm^2 with no evidence of excess heat. Yesterday, the current density on this vacuum-annealed 1-mm × 10-cm Pd cathode was raised by an order of magnitude. A Pd cathode containing about 0.2% Li (by weight) is being prepared for use in the calorimeter after the present experiment is finished, perhaps in a week or two.

The two Pd cathodes from Texas A&M University (see June 23, 1989 report) have been examined by optical microscopy. The used electrode showed a porous, dark-colored surface in contrast to the smoother and shiny surface of the unused electrode. X-ray fluorescence and diffraction measurements will be performed next.

There is nothing new or further to report on the following activities:

- Confirmation of experimental results reported by others.
- Control experiments with light water.
- Determination of mechanisms and rates for fusion reactions in solids.
- Verification of neutron and/or heat release observed in University of Utah experiments.

CJO:slg

■ The University of
ILLINOIS
at Urbana-Champaign
School of Chemical Sciences

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FACSIMILE TRANSMISSION

No. of pages: 4 Date: 7/10/89

To: Drs. John Huizenga; Norman Ramsey; Al Bard, and William Woodard

Fax no.

From: Dr. Larry R. Faulkner

Fax no. (217) 244-8068

Comments:

1-5-29721

by Huggins and coworkers normally correspond to about one half degree in temperature differential. The sensitivity of their thermocouples is about 40 microvolts per degree, hence the effect is typically represented by a signal of 20 microvolts, readable with a meter precision of about 2 microvolts per reading, or about 3 microvolts for the difference of two readings. The scatter of data points suggests that the actual precision is about half to one-third of the differential ascribed to excess heat. If there were an experimental bias inducing even a few microvolts at the thermocouple in the cell, one would see an apparent excess heat. Since the thermocouple is operated in the electric field caused by the resistance losses in solution, such an induced signal is a very real possibility. The thermocouple is encased in a glass tube containing water as a heat transfer medium, so it is assumed to be electrically isolated from the solution, but a few microvolts of pickup are not beyond the realm of possibility. Since the electric fields are larger in D_2O solutions than in H_2O solutions, this effect would explain most of the differences observed by this group between cells operated in D_2O vs. H_2O. It would also explain why the excess heat power seems to be nearly linear with applied electrolytic power.

The Huggins group has always used cells from which the gaseous electrolytic products are vented. They have not checked the volumes of vented gases to verify that recombination did not occur. The anode lead, made of Pt, extends into the head space above the solution. It could provide a catalytic site for recombination.

An interesting effect reported by this group is that the excess heat is time-dependent. They say that it does not appear at all for many hours, then it grows steadily, in some cases reaching a condition where the cell is evolving slightly more heat than could be accounted for even by full recombination. The origin of this effect is certainly not obvious, but it could be encompassed by some of the small artificial effects discussed above, if they were also time-dependent.

electrolytic mode. The group routinely applies three levels of added resistive heat at each operating point, and they measure the temperature change induced by each one. The reciprocal slope of the best straight line through the three calibration points (temperature differential vs. added power) provides a heat transfer coefficient, which is then used to calculate the electrolytic heat power from the temperature differential at that operating point in the absence of added resistive power. The Huggins group seems suitably sensitive to the need for repeated calibration. They contend that this calibration cycle proceeds for each data point on each cell. I have no reason to doubt this.

In the report of our visit to the Pons laboratory, we noted that this calibration method is intrinsically prone to overestimate the evolved heat power, because it involves the assumption that the integral heat transfer coefficient (which is actually needed for the calculation of heat power) is the same as the (calibrated) differential heat transfer coefficient at the operating point. The temperature differential is typically a nonlinear function of heat power in such a manner as to produce a positive error in the estimated heat power. Huggins is less prone to this error than Pons in one respect, because he maintains smaller temperature differentials than Pons. (Typically Huggins keeps the cell temperature below 40^o, even under the added heat power from the resistor.) On the other hand, Huggins uses rather large added heats (0-10 W) for calibration. These values are normally 20- 100 times as large as the reported excess heats. In effect, this group measures the integral heat transfer coefficient for a wide span of power above the operating point and assumes it to be equal to the integral heat transfer coefficient below the operating point. An error of a few percent in this assumption would invalidate their reports of excess heat.

In considering results from this laboratory, it is important to recognize that all electrolytic cells are operated at constant voltage, not constant current. Since the conductivity of solutions of LiOD is only about half that of corresponding solutions of LiOH, and since the cells were operated in a regime where the applied voltage appeared as a drop across the solution resistance, cells operated with H_2O electrolyzed more moles per second than equivalent cells containing D_2O. In some representations of Huggins's data (e.g. temperature differential vs. applied power for cells in the two different solvents), this effect will show apparent excess heat for the cells evolving deuterium, simply because less power is carried out of the cell in the enthalpy of formation of vented products.

A careful consideration of the measurements carried out by this group clearly illustrates the difficulty of precise measurements of evolved heat power. The excess heats report

Report on the Visit to the Huggins Laboratory at Stanford University

by Larry R. Faulkner

Date of Visit: June 6, 1989
Date of Report: June 10, 1989

I was one of several members of the Panel to have visited the laboratory of Robert Huggins at Stanford on the 6th of June. We spent roughly three hours in an intensive meeting with Huggins and three of his senior coworkers. In addition, we were able to visit the laboratory itself for about half an hour.

The meeting was structured by an informal prostration from Professor Huggins, which was interrupted as necessary by questions from the Panel members. Huggins's coworkers participated extensively in answers to the questions. In general, I felt that the group was quite open in discussing their experimental procedures and results. They seem genuinely convinced that they are seeing excess heat. I believe that this group was considerably more wary of experimental artifacts than other groups that I visited in the course of the Panel's work.

Huggins provided us with copies of nearly all of his transparencies. While this material should be considered as part of the documentary record of the visit, it is too extensive to be included with this report. My set is extensively annotated with comments and amplifications from the meeting. It is available to any interested party.

The approach used in this laboratory is similar to that used elsewhere in most respects. A cell containing a Pd cathode and a Pt anode immersed in D_2O (or H_2O) containing LiOD (or LiOH) is allowed to pass a steady current for long period of time. The cell body is immersed in a water bath, and precise measurements are made of the temperature difference between the interior of the cell and the water bath. Thermocouples and microvoltmeters are used for the temperature measurements. The cell body is not insulated with material other than the Pyrex wall, so there is relatively fast thermal relaxation. The evolved heat power is calculated from the temperature differential by a heat transfer coefficient, which is obtained by calibration via the input of additional heat power at a resistor immersed in the working solution.

The calibration method is similar to that used by Pons and coworkers, in that the resistive heat power is added to that supplied by the electrolytic process. That is, the calibration proceeds on a cell operating in a steady

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Site Visit Report: LANL July 7, 1989 (E. Storms)
T. Kenneth Fowler 7/9/89

On July 7, I visited Ed Storms at LANL for about an hour to discuss his experiments that produced tritium. The meeting was arranged through Reed Jensen who now serves as LANL cold fusion coordinator. I was actually shown around by Charles J. ("Carl") Orth on Jensen's behalf. Orth expressed skepticism about Storms' results and noted there had not yet been a LANL peer review. Official reports from June 30 and July 7 are attached.

I visited Storms at his lab and saw the setup which is still ongoing. There were a half dozen or more cells running at the time, at 3-5 volts per cell and 0.2 amp. Each cell is a 50 ml plastic bottle with rubber cork and two insulated leads to (1) a platinum semi-circular screen anode, and (2) paladium disc cathode about like a thick penny, attached to a platinum wire. I forgot to ask about the electrolyte. The main thrust of the work has been to study D/Pd ratios vs. surface treatment of the Pd. The Pd discs are fused at LANL by Storms and coworkers from Pd powder. They find high voltages (but ≤ 5 volts, I think) and higher D/Pd ratios if surfaces are treated (eg. "sulfided" in H_2S environment) - with ratios exceeding unity, up to 1.3 (volume average, measured by weighing loaded Pd disc vs. disc after driving out the D). The cells "producing" tritium were sulfided, I believe, and reached D/Pd ~ 1.0; their charging histories were recorded.

The main points I learned were:

1. Only 2 out of 51 cells have shown tritium. Storms has not been able to repeat it despite many attempts
2. The amount observed was 100 times background ($\geq$ 4000/ml/minute; background 35 for this D_2O)
3. Contamination cannot be ruled out
   (1) Tritium was observed about 10 days into the run
   (2) There were no other measurements prior to this to give a buildup history: insofar as direct measurements are concerned, tritium could have been there all along
   (3) Shortly after detecting tritium, the polarity was reversed to make Pd the anode and "drive out any remaining tritium in the Pd" — none came out, the tritium level in D_2O did not increase (Storms interprets this as evidence of surface production but it could equally well mean no production in situ)
4. Other facts
   (1) The experiment is located in the TSTA facility where megacuries of tritium exist
   (2) Pd discs are prepared in this building
   (3) The 2 discs/cells that showed tritium were prepared at the same time (others at different times)
   (4) Jim Anderson, TSTA Manager, doubts contamination from his activities which are carefully monitored. However, per my discussion with him, we concluded that the amount in question — 10^{13} atoms in a 50ml cell showing 4000/ml/min (= 10^{-8} Curie) — is small. For example, a "swipe" showing this count rate would call for cleaning it up but would not constitute a reportable incident

Los Alamos
Los Alamos National Laboratory
Los Alamos,New Mexico 87545

memorandum

TO: Reed Jensen, ADR, MS-A114
DATE: June 30, 1989
FROM: Charles Orth, INC-11, MS-J514
MAIL STOP/TELEPHONE: J514/7-4785
SYMBOL: M331TX9
SUBJECT: REPORT ON "COLD FUSION" ACTIVITIES AT LOS ALAMOS - June 24 to June 30, 1989

An experiment to test the reproducibility of generating small neutron bursts from a pressurized D_2 gas bottle containing Titanium-662 alloy (Ti with 6% Al, 6% V and 2% Sn) at room temperature produced four short-lived bursts ranging from 30 to 100 neutrons from the source. Plans are now underway to run normal hydrogen control experiments and to use DT gas as soon as the safety aspects can be resolved.

In an earlier report it was noted that small bursts of neutrons were detected from a group of six Jones-type electrochemical cells with V, Pd or fused Pd-Ti alloy cathodes, being operated simultaneously within a neutron well counter. This week a single Pd-Ti alloy cell was run in the well counter but neutron bursts were not observed. This suggests, but does not prove, that this type of cathode was not responsible for the previously reported bursts.

A Pd cathode with a sulfided surface was prepared in the same manner as the two cathodes that appeared to previously produce tritium, and it was electrolyzed while in a neutron well counter. Neither significant tritium buildup nor excess neutrons was observed. However, a thin deposit of lead and copper from dissolution of a solder joint at the top of the cell was later found on the surface of this cathode. The cathode leads and soldered connectors of the two cells in which tritium buildup was reported were coated with a Torr Seal epoxy protective coating. Three additional sulfided electrodes with Torr Seal coatings are currently being studied. One of these is being electrolyzed in a neutron well counter in the low-background neutron counting laboratory. All three are being monitored for tritium buildup. If a significant increase in tritium is observed in the neutron counting laboratory experiment, the cell will be removed from the well counter and then be measured with a sensitive x-ray counter. If there is a significant increase in the tritium concentration, copious numbers of 21-keV Pd K x-rays should be produced via interaction of protons from the ^{3}T-p fusion channel with the Pd atoms.

In materials studies, Pd single crystals have shown a reluctance to charge to D/Pd ratios greater than about 0.7 and then to discharge back to D/Pd ratios less than about 0.6. In another experiment a Pd-Rh-Li alloy cathode has been charged to D/Pd = 1.29 and then placed in liquid nitrogen awaiting neutron diffraction studies.

A calorimeter with oxygen-deuterium gas recombination within the electrolysis cell has been constructed and a measurement with a 1 mm x 10 cm vacuum annealed Pd cathode is in progress. The calorimeter has a heat output over input sensitivity of about 1%. Resistivity of the cathode is constantly monitored to measure the degree of D loading. Neutron and gamma-ray emission from the cell are also being continuously monitored with a NE-213 scintillation detector using pulse shape discrimination to distinguish between the two types of nuclear radiation. Tritium will be assayed in the input and final electrolyte solutions.

Some theoretical work is being directed to the possibility of resonance-enhanced deuteron Coulomb barrier penetration.

There is nothing further or new to report on the following activities:

- Confirmation of experimental results claimed by others.
- Chemical assays of chemical reactants and products.
- Control experiments with light water.
- Determination of mechanisms and rates for fusion reactions in solids.
- Verification of neutron and/or heat release observed in the University of Utah experiments.

CJO:cel

cy: INC-11 File

Los Alamos

Los Alamos National Laboratory
Los Alamos, New Mexico 87545

memorandum

TO: Reed J. Jensen, ADR/A114

DATE: July 7, 1989

FROM: Charles Orth, INC-11 CJO

MAIL STOP/TELEPHONE: J514 / 7-4785

SYMBOL: ADR:89-292

SUBJECT: **REPORT ON "COLD FUSION" ACTIVITIES AT LOS ALAMOS—July 1 to July 7, 1989**

The three sulfide-coated Pd cathodes with Torr Seal protected leads and solder joints described in the last progress report have been electrolyzed for a week. The current densities in two of these were raised from 40 to 200 mA/cm^2 during this time interval, and thus far, no significant tritium buildup has been observed. The third unit is being electrolyzed at 40 mA/cm^2 while in a neutron well counter at the low-background laboratory. Small aliquots of electrolyte have been removed for tritium assay, yet to be done, and only background neutron rates have been observed.

In solid-state, D_2-gas experiments, eight Ti cathodes from Jones-type electrolysis cells were cycled together several times between liquid nitrogen and room temperature under 40 atmospheres of deuterium before a single burst of about 100 source neutrons was detected. The burst occurred during a warmup at about −30°C. Experience gained from work on the gas-metal systems indicates that 2 to 4 days of "incubation" under 40 atmospheres D_2 pressure (room temperature) is an essential step toward obtaining the previously observed neutron bursts. Although Ti-662 alloy appears to yield reproducible neutron bursts in high-pressure systems (40 atmospheres D_2), a recent experiment to determine low-pressure (3 atmospheres) behavior failed to yield neutrons. A second low-pressure experiment with pre-conditioning of the Ti-662 surface will be performed.

The effects of carbon, sulfur, and trace metal impurities on deuterium takeup in Pd are being explored in a systematic manner. Measurements in progress indicate that minor impurities have a profound effect.

The calorimeter experiment described in last week's report has been "charging" for one week at a current density of 60 mA/cm^2 with no evidence of excess heat. Yesterday, the current density on this vacuum-annealed 1-mm × 10-cm Pd cathode was raised by an order of magnitude. A Pd cathode containing about 0.2% Li (by weight) is being prepared for use in the calorimeter after the present experiment is finished, perhaps in a week or two.

The two Pd cathodes from Texas A&M University (see June 23, 1989 report) have been examined by optical microscopy. The used electrode showed a porous, dark-colored surface in contrast to the smoother and shiny surface of the unused electrode. X-ray fluorescence and diffraction measurements will be performed next.

There is nothing new or further to report on the following activities:

- Confirmation of experimental results reported by others.
- Control experiments with light water.
- Determination of mechanisms and rates for fusion reactions in solids.
- Verification of neutron and/or heat release observed in University of Utah experiments.

CJO:slg

✓

THE
UNIVERSITY
OF UTAH

June 22, 1989

Dr. Norman Ramsey
Professor of Physics
Williams College
Williamstown, MA 01267

Dear Dr. Ramsey:

I just learned that one of the DOE team members verbally rejected our data out of hand in the presence of Dr. J. O'M Bockris. I consider this very unprofessional. This team member reportedly "trashed" our data on the basis that there was no "tell-tale" decrease in voltage. As you know, I called you the day after the DOE visit to Utah and reported to you that the run in question had been operated at a constant voltage setting. I was unaware of that point at the time of the visit. Why this point wasn't clarified then I'm not sure. All runs prior to May 5 were constant voltage runs with hand recording of current. After May 5 we changed to a constant current mode. Evidently the team, or at least this team member, does not believe what we say. I find this offensive, unfair and hardly in tune with effective evaluation.

In other runs, operated at constant current and more thoroughly instrumented, we have noted the expected decrease in voltage. We have 3 subsequent runs with such results. The results we are getting are important and growing both in quantity and sophistication. We have found an increase in tritium in the rod experiencing the heat excursions we showed you. Before outgassing of the cathode the tritium was 37% above background. After outgassing in the cathode the level increased to between 300 and 400 percent of background based on our original solution. This analysis has been checked by two separate laboratories.

Sincerely yours,

Milton E. Wadsworth

MILTON E. WADSWORTH
Professor of Metallurgy
and Dean

cc:	Dr. Barry Miller	Dr. Hugo Rossi
	Dr. Allen Bard	Dr. James Brophy
	Dr. William Happer	Dr. Stanley Pons
	Dr. Larry Faulkner	Dr. Martin Fleischmann
	Dr. Gerry Byrne	Dr. Sivar Guruswamy

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Lyman Physics Laboratory
Harvard University
Cambridge, MA 02138

July 8, 1989

Dean Milton Wadsworth
209 W. C. Browning Building
The University of Utah
Salt Lake City, Utah 84112-1183

Dear Dean Wadsworth:

I am sorry to be so late in replying to your letter of June 22, 1989, but it was addressed to me at Williams College and suffered forwarding delays as well as being delivered when I was out of town. As a result I received you letter only late yesterday.

From your letter it is difficult to infer which Panel member you are referring to and exactly what was said, however, I can add some information as to whether said Panel member at the time knew of the phone call I received from your laboratory following our visit to Utah. There was no meeting of our committee between the time I received the call from your laboratory and our visit to Texas so I distributed information about this call in writing to all members of the Committee. However, I had to leave on a trip immediately following the call from your lab and I returned from the trip only onJune 13. As a result, the report went out on Wednesday June 14,1989. Since we left for Texas on Sunday June 17, it is highly likely that most Panel members had not received much less read that report prior to the Texas visit.

There seems to be some misunderstanding between the two of us on two matters pertaining to the phone call and I welcome this opportunity to have them clarified. You are quite explicit in stating "As you know, I called you the day after the DOE visit to Utah and reported to you ...". It was my understanding that the call was not from you but from your associate Dr. Guruswamy and in my report to the Committee I said the report was from him. Although I do not have good records on the timing of the call, I believe I received it two or three days following our visit rather than "the day after". Also your letter implies that the run in question had been deliberately opererated at a constant voltage setting whereas it was my understanding from Dr. Guruswamy that it was realized following our visit that the power supply had automatically shifted to constant voltage when the voltage exceeded a certain limit.

If you would like to supply any clarification or further details on these or other matters please feel free to do so. Since I shall be away for a considerable period of time, I suggest that you send such information to Mr. William Woodard, Secretary, Cold Fusion Panel, ER-6, 3F-043, US Department of Energy, 1000 Independence Avenue, SW, Washington, DC 20585, FAX: 202-586-3119, with the request that he distribute it to all members of the Panel.

Thank you for the report on your new information. I am distibuting copies of your letter to all members of the panel. If any futher new relevant information develops, please send it to Mr. W. Woodard with the request that it be distributed to the Panel members.

Sincerely yours,

Norman F. Ramsey
Higgins Professor

13A

Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

-R.L. GARWIN-
26 JUN 89 12. 38

June 23, 1989

TO: Cold Fusion Panel

Enclosed is the Site Visit to Texas A&M from David Goodwin, Report on visit to Cold Fusion Research at ORNL, and Cold Fusion News No. 15, dated 17 June, 1989.

Bill
William Woodard

Enclosures

062389.WLW

DOE 1325.8

United States Government **Department of Energy**

memorandum

DATE: JUN 21 1989

REPLY TO ATTN OF: ER-20.1

SUBJECT: June 19, 1989, Site Visit to Texas A&M

TO: W. Woodard, ER-6

In attendance were Huizenga (Co-Chairman from Rochester), Ramsey (Co-Chairman from Harvard), Bard (Texas), Bigeleisen (Stony Brook), Callis (ACS President), Happer (Princeton), Miller (AT&T), Schiffer (Argonne), Wrighton (MIT), Finn (ERAB) and myself (Technical Advisor).

Group #1

Appleby provided attachments #1 and #2 on the following experiments:

1. Three experiments with the following excess heat generation:
    (A). 3 runs: 16.3, 19.3 18.5 watts/cc
    (B). 4-7 watts/cc
    (C). 6-12 watts/cc
2. Two experiments where:
    (A). Excess heat generation was nearly eliminated by switching from LiOD to NaOH and
    (B) Excess heat generation returned by switching back to LiOD.
3. An experiment where excess heat generation was nearly eliminated by switching from an uncoated stainless steel cell to a teflon-coated Ni cell.
4. An experiment with LiOH, instead of LiOD, which generated no excess heat.
5. An experiment with Pt in LiOD, instead of Pd in LiOD, which generated no excess heat.
6. An experiment where:
    (A). Excess heat generation was nearly eliminated by switching from 6% Li-6 to Li-7 and
    (B). Excess heat was generated faster by switching to Li-6.

Analysis shows Li penetrates about 1 micron into the electrode. Surface analysis, at least some of which was done by Livermore, indicates Fe, Cr, Mg, Ca and Si. Bockris noted that Ni is also present.

Appleby also noted they have recently began measuring gas volumes in one experiment to confirm low levels of recombination.

Group #2

Wolf provided 3 positive results on neutrons from the Texas A&M, including 2.5 MeV neutrons up to 4 1/2 times background. The highest neutron level corresponded to an over 10^3 tritium increase. He also presented surface analysis which indicated (probably Fe based) dendrites and cracks. He also discussed the possibly of D + Li-6 fusion.

Group #3

Packham, from the Bockris group, provided attachment #3 and discussed 8 positive results for tritium, showing increases up to $> 10^4$ and that tritium analysis have been confirmed by sets of 5 samples which were sent to Argonne, Pacific Northwest, Los Alamos and General Motors.

Bockris provided attachment #4 and discussed:

(1) His 3 cells with excess heat: 4.5, 5.0 and 7 watts/cc,
(2) That he is installing recombiners in 1/2 of his cells,
(3) That his calculations from Henry's Law indicate about 100 times more tritium in the gas than in the electrolyte, which is approximately the level of tritium suggested by the excess heat, and
(4) That Fe/Cr and Ni dendrite tips may produce the KeV energies required for fusion.

Group #4

Martin reported on :

(1) Two cells, one with up to 50% excess heat,
(2) That measurements of gas flow indicated recombination does not exceed 1%, even for exposed electrodes
(3) Collaboration with Vancouver to use their flow-thru calorimeter with a recombiner, and
(4) Surface analysis shows high levels of Li and increased levels of C, Si and O.

Tours were then provided by each of the four groups.

ERAB Panel members have requested that copies of this summary of what was reported not be distributed beyond the ERAB Panel and the DOE distribution list.

David Goodwin
Special Assistant
Associate Director for
High Energy and Nuclear Physics

Attachment (4)

cc: W. Hess, ER-20
P. Stone, ER-1
T. Finn, ER-55
R. Gajewski, ER-16
L. Ianniello, ER-11
D. Barney, ER-16
R. Marianelli, ER-14
D. Crandall, ER-54
R. Marley, ER-1
W. Warnick, ER-32
D. Hendrie, ER-23
C. Richardson, ER-23

EVIDENCE FOR EXCESS HEAT GENERATION RATES DURING ELECTROLYSIS OF D_2O IN LiOD USING A PALLADIUM CATHODE - A MICROCALORIMETRIC STUDY

A. John Appleby, Supramaniam Srinivasan,
Young Jin Kim and Oliver J. Murphy

Center for Electrochemical Systems and Hydrogen Research
Texas Engineering Experiment Station
Texas A&M University
College Station, Texas 77843

Presentation to
U.S. Department of Energy "Cold Fusion" Task Force Review
Monday, June 19, 1989

PRINCIPLES OF MICROCALORIMETER
TRONAC MODEL 350

* The design is based on the Seebeck effect in which a voltage is produced proportional to the temperature difference across the thermoelectric device (TED).

* The voltage generated across the TED is directly proportional to the heat flow from the microcalorimeter measuring cell.

* The rate of heat generation from 1μW to 8W can be measured.

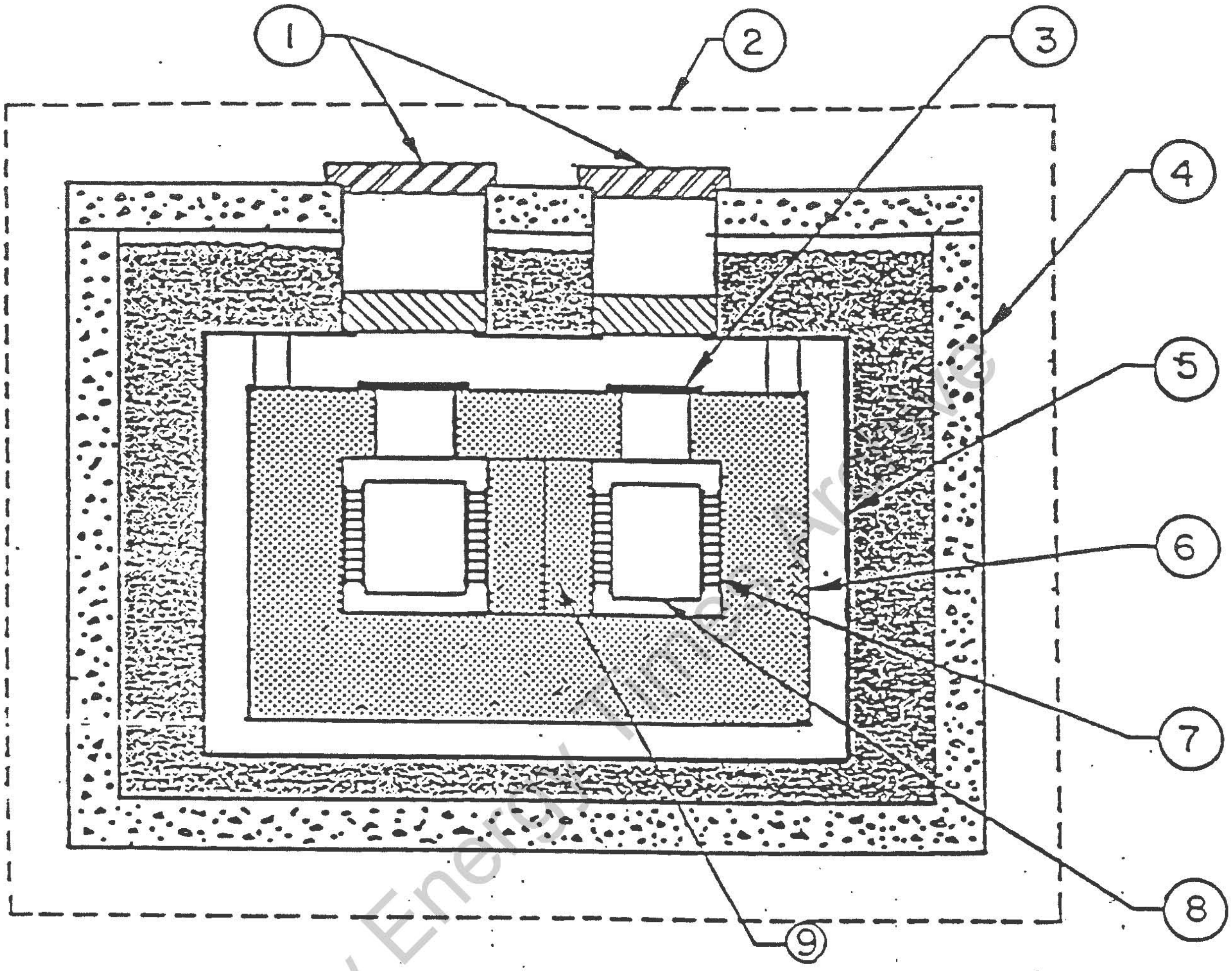


Figure 2: Schematic of Tronac Calorimeter System

(1) Access lid
(2) Temperature-controlled air bath.
(3) Cover on block compartment
(4) Temperature-controlled water bath
(5) Water-tight stainless steel box.
(6) Aluminum heat sink block.
(7) Thermoelectric sensors.
(8) Sample holder.
(9) One of four aluminum wedges placed in tapered channel.

EXPERIMENTAL APPROACH

* MATERIAL

Pd: (i) 0.5mm diameter x 10mm long
(ii) 1.0mm diameter x 10mm long
(iii) 2 mm diameter sphere

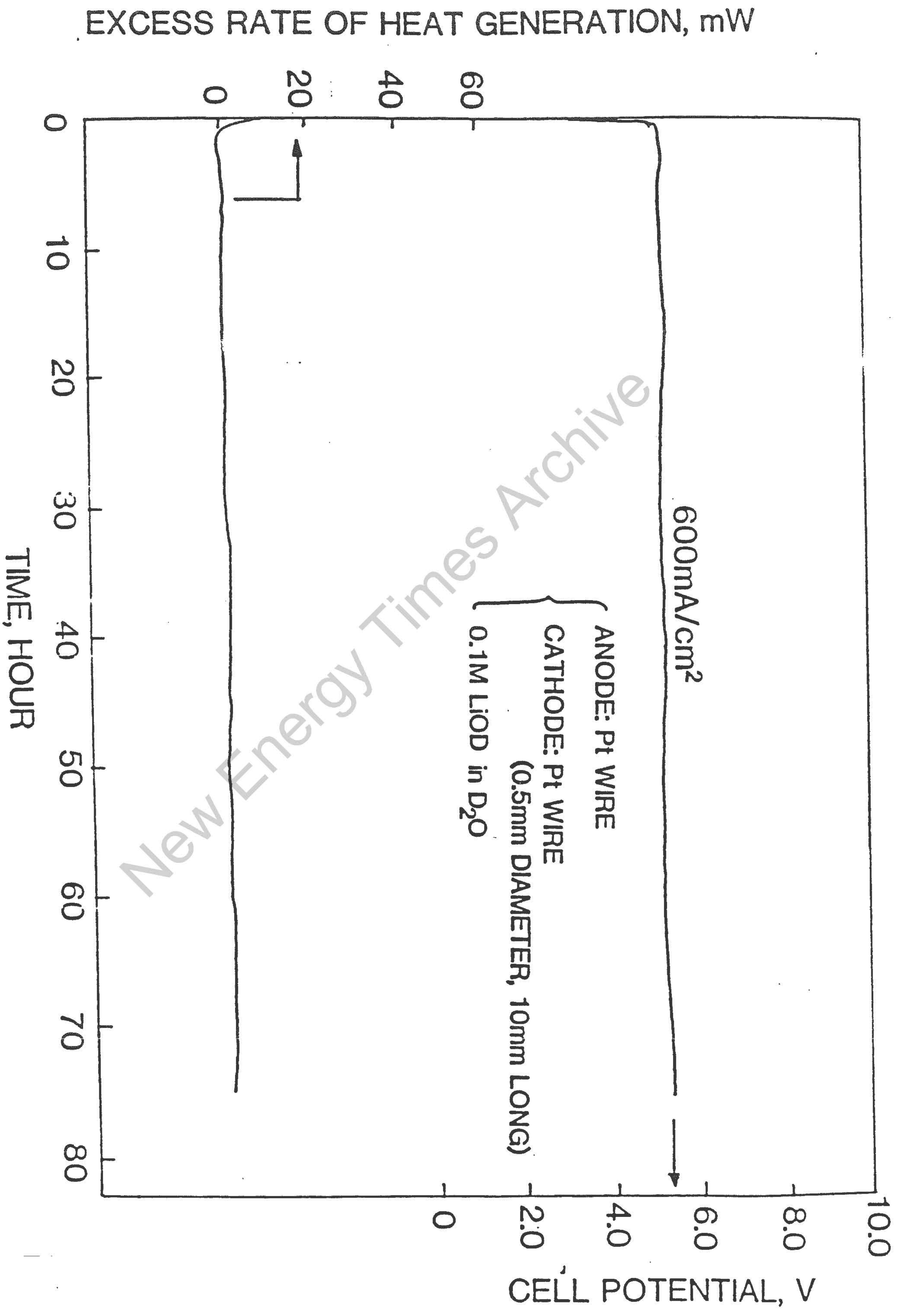
Pt: (i) 0.5mm diameter x 10mm long
(ii) 1.0mm diameter coil

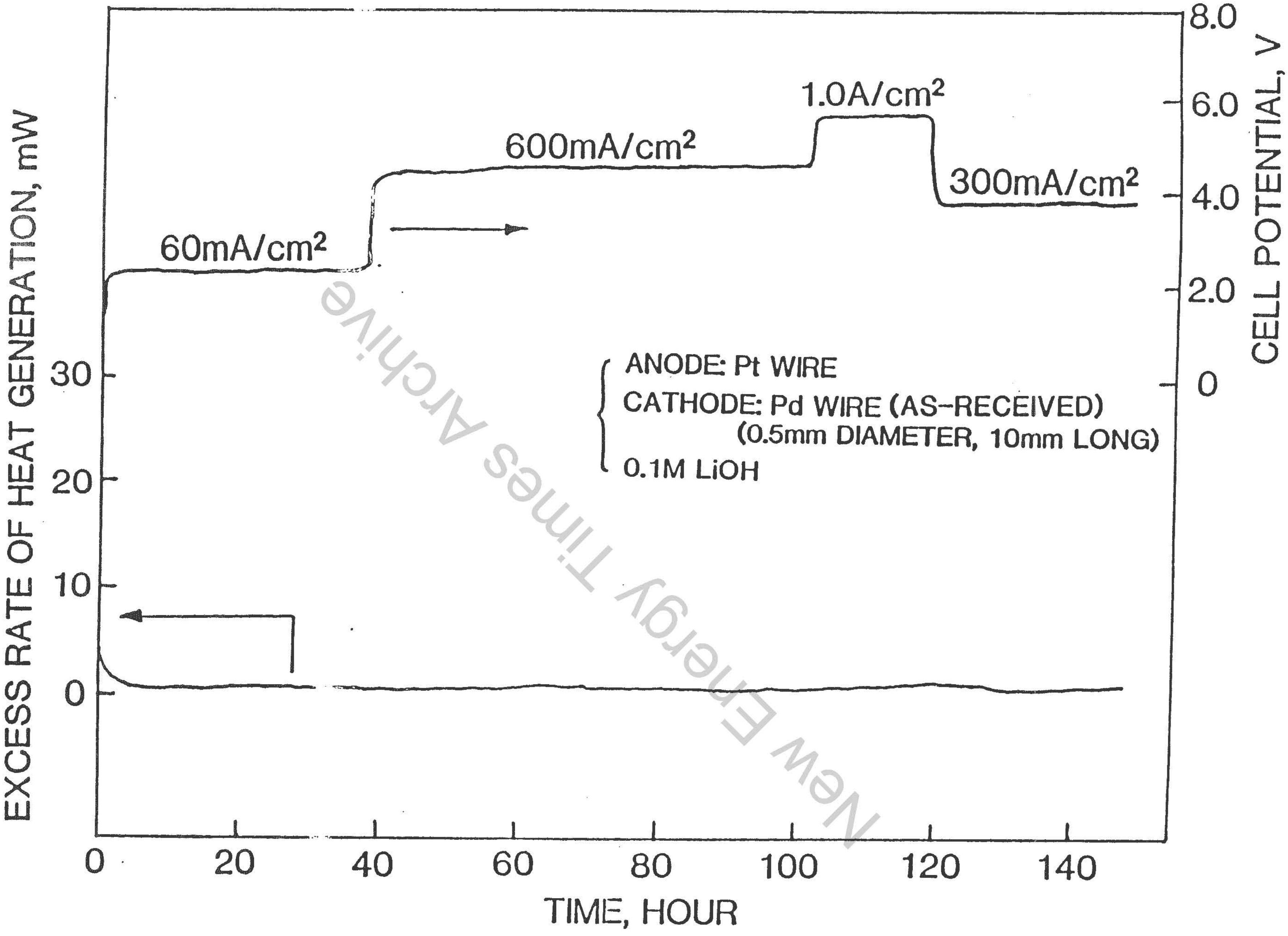
*ELECTROLYTE

(i) 0.1M LiOD, 0.1M LiOH, 0.1M NaOD, 1.0M LiOD, 0.,1M ^{6}LiOD, and 0.1M ^{7}LiOD

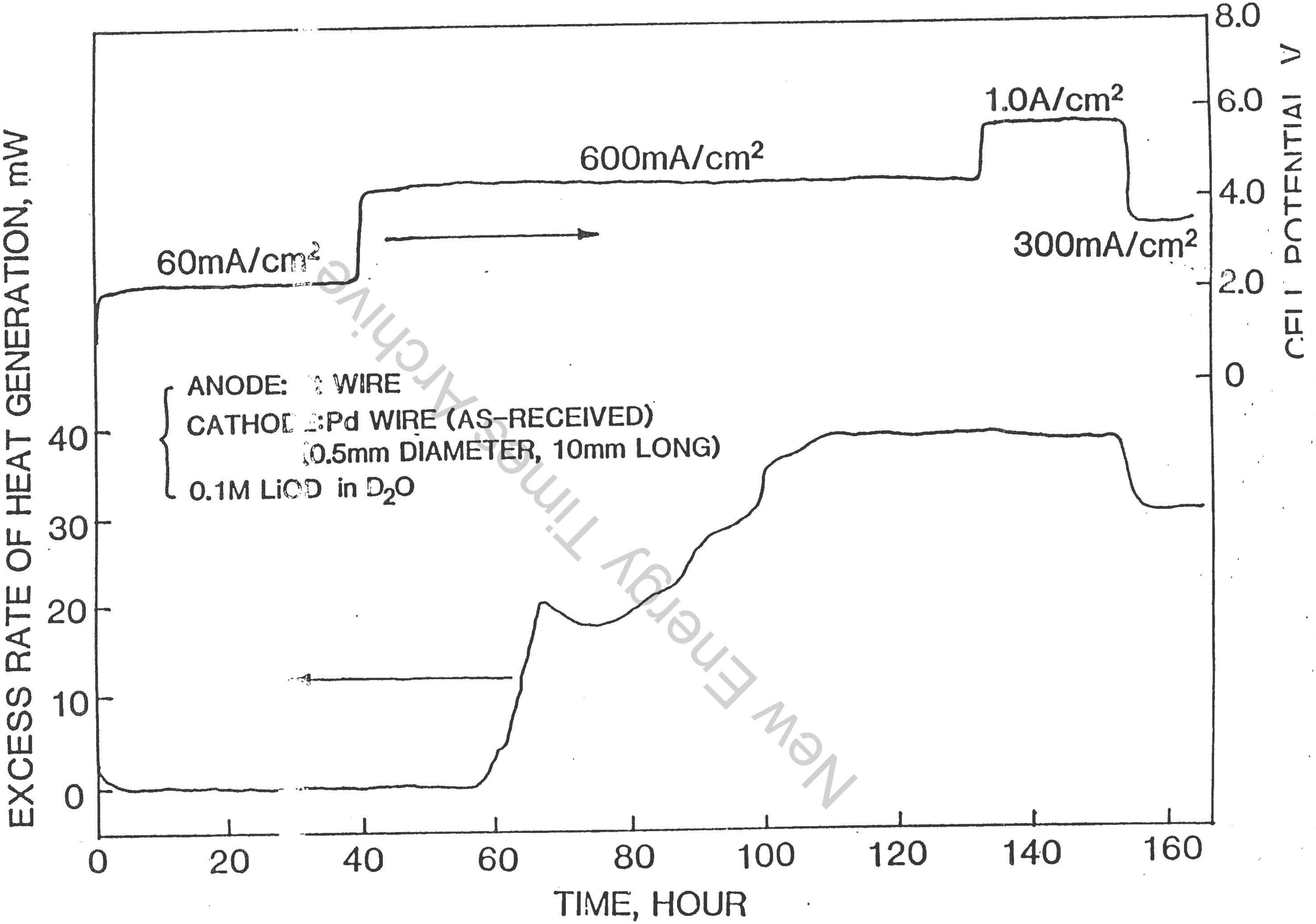
(ii) Volume: 7.5 - 8.0 ml.

*The rate of heat generation was measured in a closed stainless steel (1cm x 3.9cm x 3.5m). The rate of heat generated by oxidation of the stainless steel in the electrolyte is negligible.


EXCESS RATE OF HEAT GENERATION, mW
0
20
40
60
600mA/cm²
ANODE: Pt WIRE
CATHODE: Pt WIRE
(0.5mm DIAMETER, 10mm LONG)
0.1M LiOD in D₂O
TIME, HOUR
0
10
20
30
40
50
60
70
80
0
2.0
4.0
6.0
8.0
10.0
CELL POTENTIAL, V


EXCESS RATE OF HEAT GENERATION, mW
0
10
20
30
CELL POTENTIAL, V
0
2.0
4.0
6.0
8.0
TIME, HOUR
0
20
40
60
80
100
120
140
60mA/cm²
600mA/cm²
1.0A/cm²
300mA/cm²
ANODE: Pt WIRE
CATHODE: Pd WIRE (AS-RECEIVED)
(0.5mm DIAMETER, 10mm LONG)
0.1M LiOH

V.C.


EXCESS RATE OF HEAT GENERATION, mW
0
10
20
30
40
TIME, HOUR
0
20
40
60
80
100
120
140
160
CELL POTENTIAL, V
0
2.0
4.0
6.0
8.0
60mA/cm²
600mA/cm²
1.0A/cm²
300mA/cm²
ANODE: WIRE
CATHODE:Pd WIRE (AS-RECEIVED)
(0.5mm DIAMETER, 10mm LONG)
0.1M LiOD in D₂O

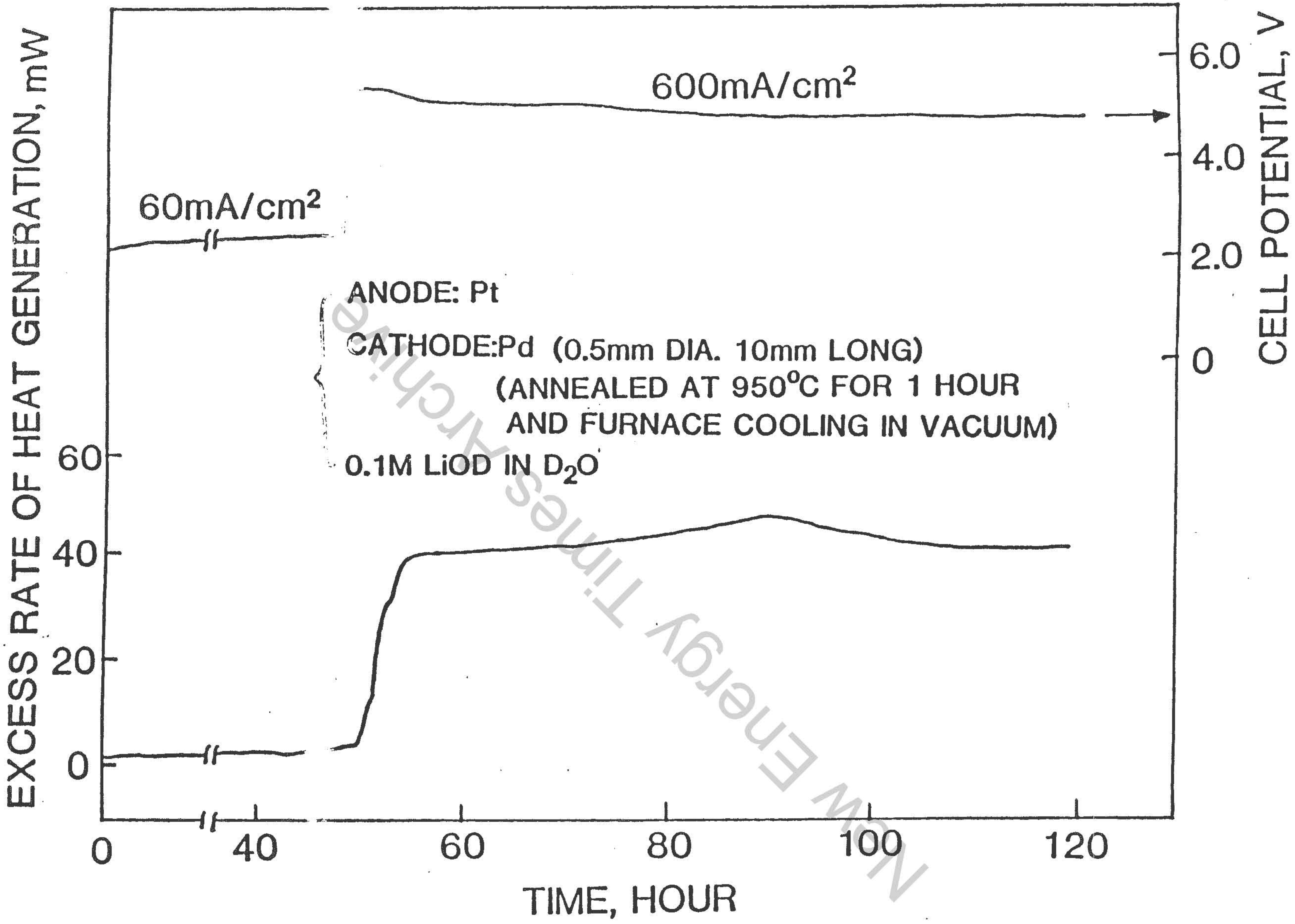
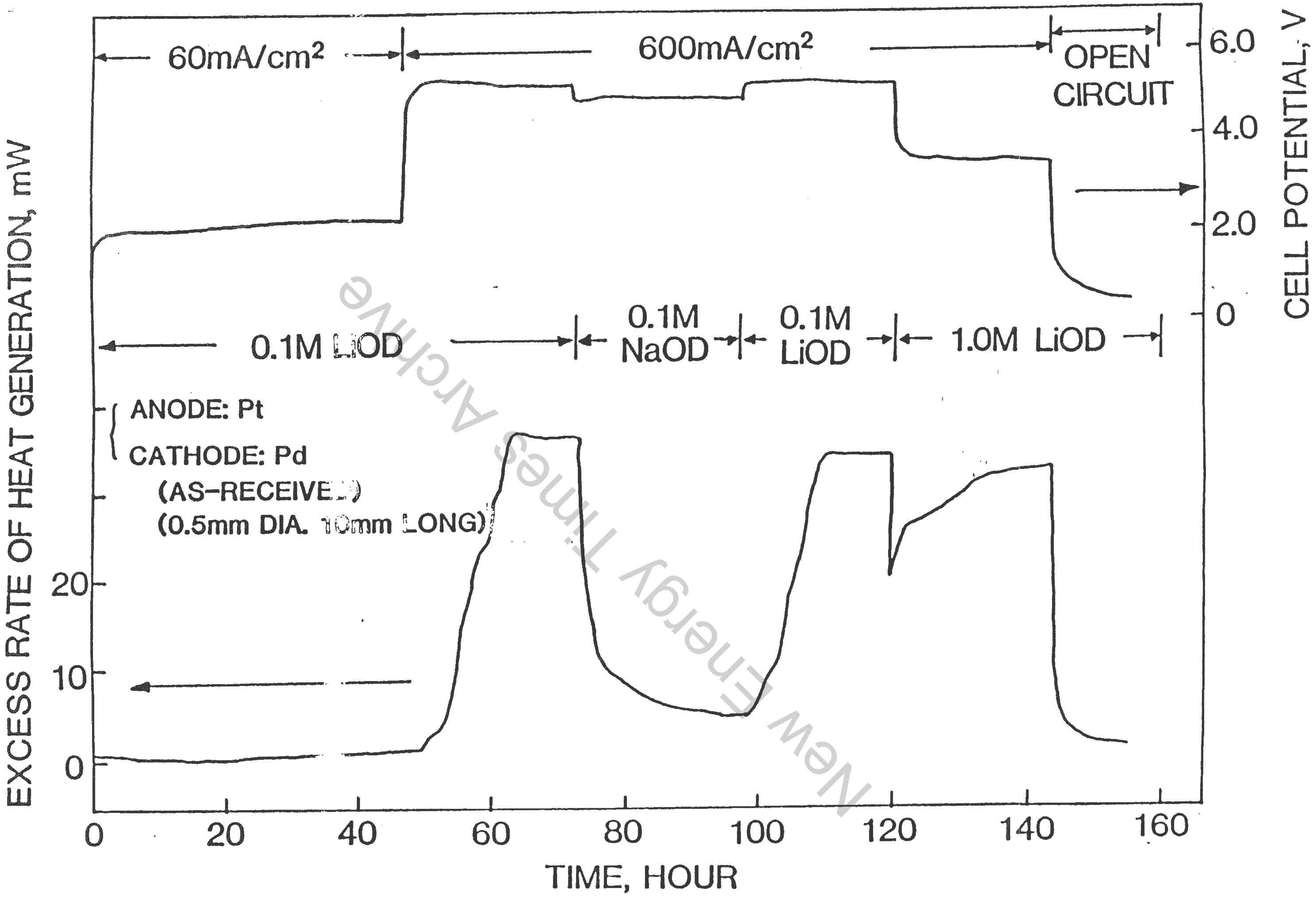

EXCESS RATE OF HEAT GENERATION, mW
0
20
40
60
60mA/cm²
600mA/cm²
ANODE: Pt
CATHODE:Pd (0.5mm DIA. 10mm LONG)
(ANNEALED AT 950°C FOR 1 HOUR
AND FURNACE COOLING IN VACUUM)
0.1M LiOD IN D₂O
6.0
4.0
2.0
0
CELL POTENTIAL, V
0
40
60
80
100
120
TIME, HOUR

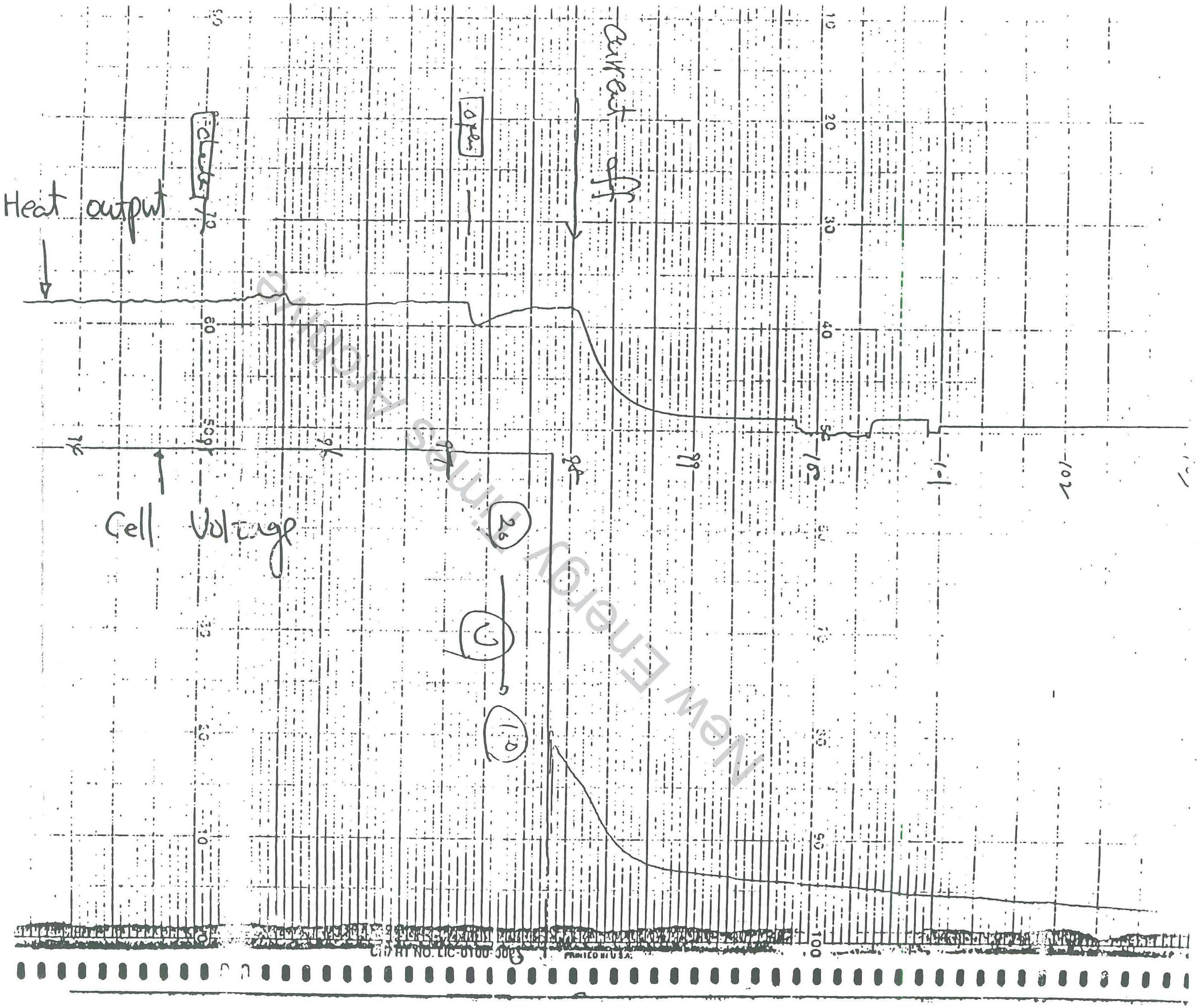
TABLE: Evidence for "Anamolous Heat Generation" During Electrolysis of D2O in LiOD on Palladium Cathode - Results of Test and Control Experiments

Exp. #	Electrode Material		Electrolyte	Current Density	Rate of Excess Heat Generation
	Cathode	Anode		mA/cm*2	W/cm*3 of Pd
1	Pd 0.5mm dia. 10mm long	Pt	0.1M LiOD	300	16.3
				600	19.3
				1000	18.5
2	Pd 0.5mm dia. 10mm long	Pt	0.1M LiOH	600	0
3	Pt 0.5mm dia. 10mm long	Pt	0.1M LiOD	600	0
4	Pd 1.0mm dia. 10mm long	Pt	0.1M LiOD	600	4 - 7
5	Pd 2.0mm dia. sphere	Pt	0.1M LiOD	600	6 - 12

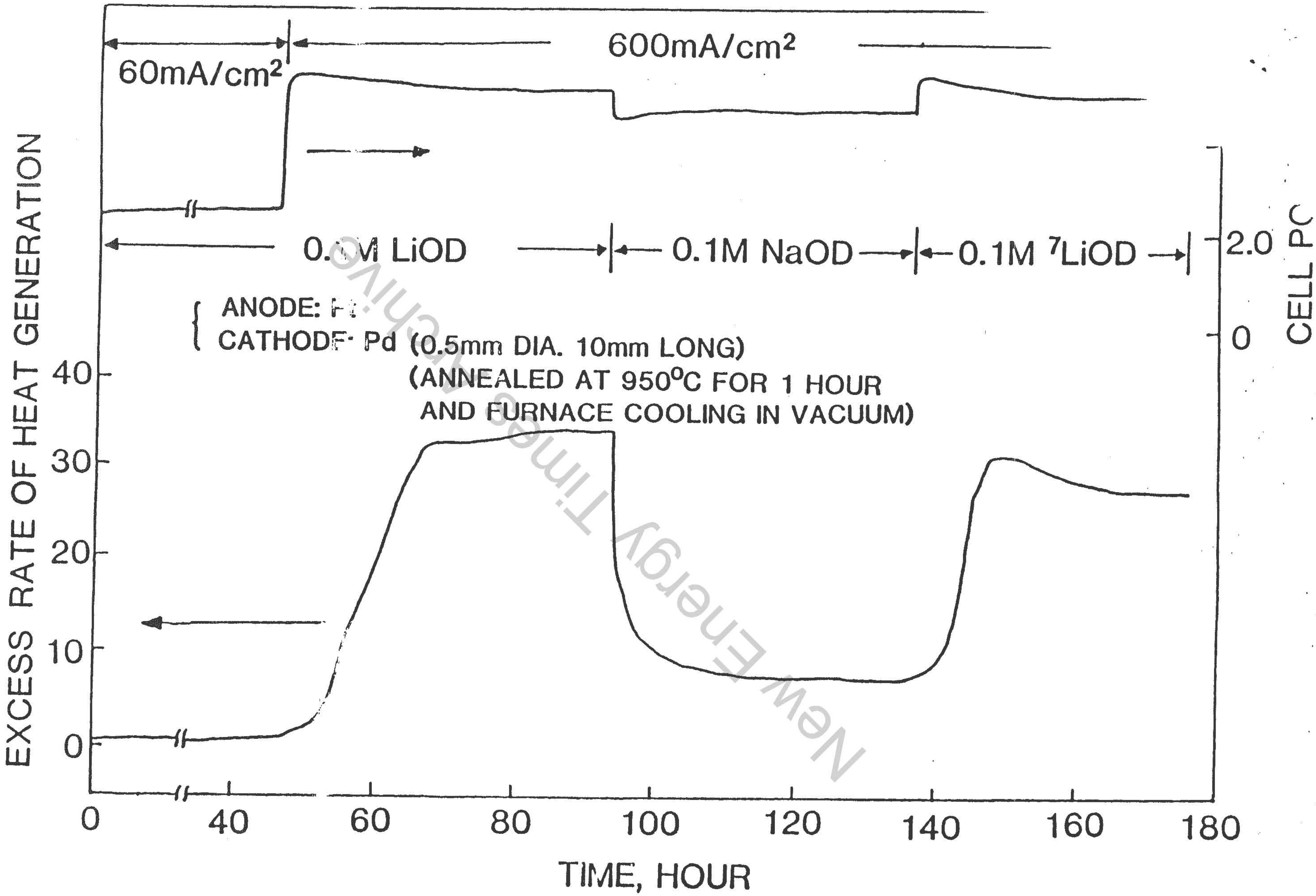
VG 9


60mA/cm²
600mA/cm²
OPEN CIRCUIT
0.1M LiOD
0.1M NaOD
0.1M LiOD
1.0M LiOD
ANODE: Pt
CATHODE: Pd
(AS-RECEIVED)
(0.5mm DIA. 10mm LONG)
EXCESS RATE OF HEAT GENERATION, mW
0
10
20
CELL POTENTIAL, V
0
2.0
4.0
6.0
0
20
40
60
80
100
120
140
160
TIME, HOUR

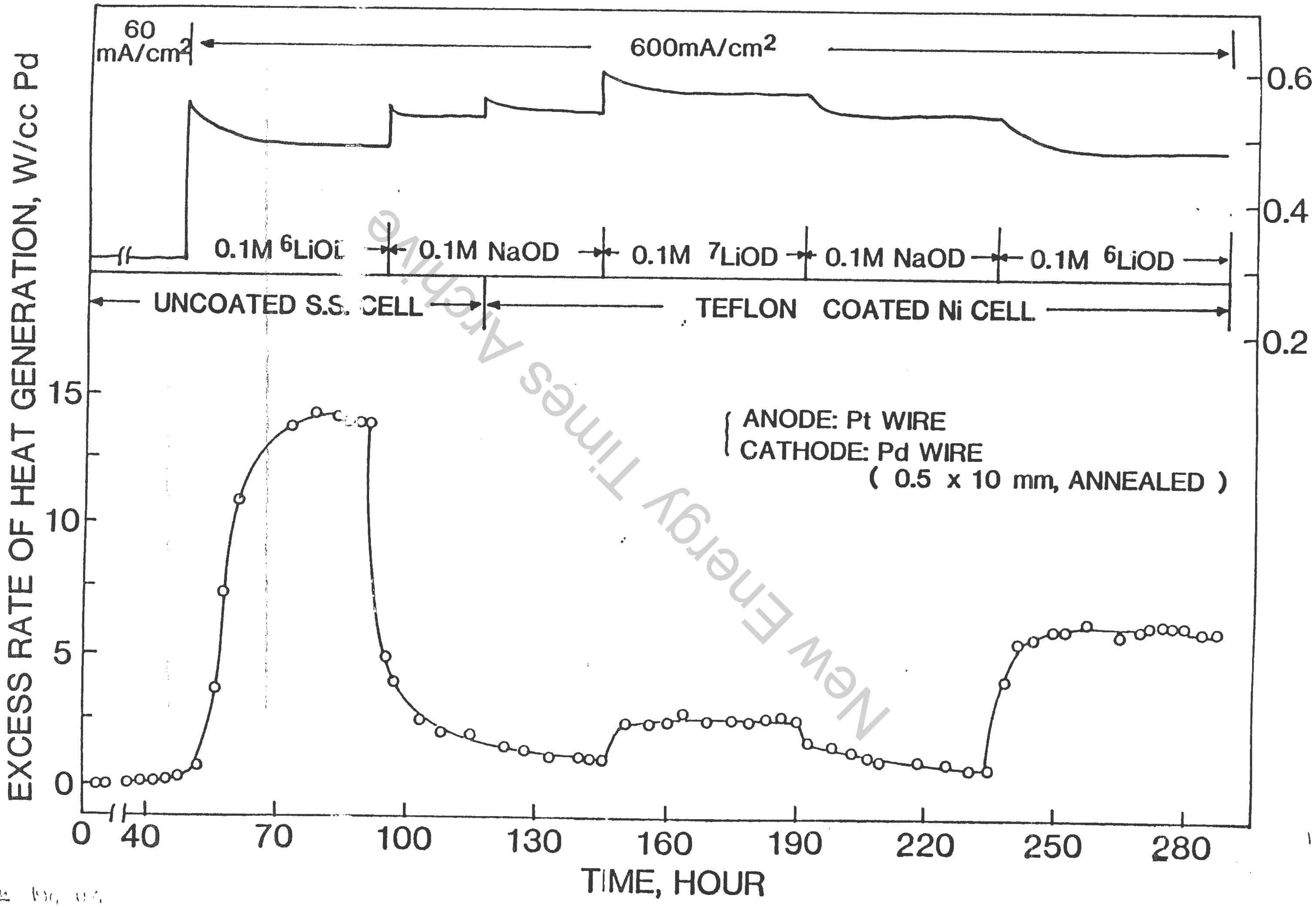
VG. 1b


Heat output
closed
open
Current off
Cell Voltage
99
101
102
10
20
30
40
60
70
100
CHART NO. LIC-0100-3065
PRINTED IN U.S.A.

V.G. 11


60mA/cm²
600mA/cm²
0.1M LiOD
0.1M NaOD
0.1M ⁷LiOD
ANODE: Pt
CATHODE: Pd (0.5mm DIA. 10mm LONG)
(ANNEALED AT 950°C FOR 1 HOUR
AND FURNACE COOLING IN VACUUM)
EXCESS RATE OF HEAT GENERATION
0
10
20
30
40
CELL PO
0
2.0
0
40
60
80
100
120
140
160
180
TIME, HOUR

VG 12


EXCESS RATE OF HEAT GENERATION, W/cc Pd
0
5
10
15
0.2
0.4
0.6
60 mA/cm2
600mA/cm2
0.1M 6LiOD
0.1M NaOD
0.1M 7LiOD
0.1M NaOD
0.1M 6LiOD
UNCOATED S.S. CELL
TEFLON COATED Ni CELL
ANODE: Pt WIRE
CATHODE: Pd WIRE
(0.5 x 10 mm, ANNEALED)
0
40
70
100
130
160
190
220
250
280
TIME, HOUR

[illegible]

V.G 12

HELIUM CONCENTRATIONS IN COLD FUSION PALLADIUM

Sample	A&M Group	Description	Sample Mass[a] (mg)	Measured Helium (atoms released)	
				^3He	^4He
JOMB-Pd1-A	Bockris	1 mm ø wire test sample	11.72	$<1.2 \times 10^9$	$<0.3 \times 10^9$
-B			13.56	$<0.7 \times 10^9$	$<0.2 \times 10^9$
JOMB-Pd2-A	Bockris	1 mm ø wire control	14.19	$<1.2 \times 10^9$	$<0.3 \times 10^9$
-B			14.49	$<0.7 \times 10^9$	$<0.2 \times 10^9$
JAAM-Pd1-A	Appleby	1 mm ø wire control	9.14	$<1.2 \times 10^9$	$<0.3 \times 10^9$
-B			10.74	$<0.7 \times 10^9$	$<0.2 \times 10^9$
JAAM-Pd2-A	Appleby	2 mm ø sphere test sample	9.18	$<1.2 \times 10^9$	$<0.3 \times 10^9$
-B			8.79	$<0.7 \times 10^9$	$<0.2 \times 10^9$

[a] Mass uncertainty is ±0.01 mg.

Vc. 14

```
[illegible]
[illegible]: [illegible]
TIME:      60         WINDOW WIDTH:    10        STEP:    10

   WINDOW          CPM   0.0       2.0       4.0       6.0       8.0      10.0  E 0
                         +---------+---------+---------+---------+---------+------
    5 -   14        .00 *
   15 -   24        .00 *
   25 -   34        .00 *
   35 -   44        .00 *
   45 -   54        .00 *
   55 -   64       1.00 I====*
   65 -   74       6.00 I=============================*
   75 -   84       2.00 I=========*
   85 -   94       7.00 I==================================*
   95 -  104       6.00 I=============================*
  105 -  114      10.00 I=================================================*
  115 -  124       7.00 I==================================*
  125 -  134       5.00 I========================*
  135 -  144       9.00 I============================================*
  145 -  154       5.00 I========================*
  155 -  164       4.00 I===================*
  165 -  174       1.00 I====*
  175 -  184       1.00 I====*
  185 -  194       2.00 I=========*
  195 -  204       1.00 I====*
  205 -  214        .00 *
  215 -  224        .00 *
  225 -  234        .00 *
  235 -  244        .00 *
  245 -  254       1.00 I====*
  255 -  264       1.00 I====*
  265 -  274        .00 *
  275 -  284       4.00 I===================*
  285 -  294        .00 *
-----------------------------
    5 -  294      73.00
```

After Test

```
SAMPLE SPECTRUM
POSITION: 002
TIME:      60         WINDOW WIDTH:    10        STEP:    10

   WINDOW          CPM   0.0       2.0       4.0       6.0       8.0      10.0  E 0
                         +---------+---------+---------+---------+---------+------
    5 -   14        .00 *
   15 -   24        .00 *
   25 -   34        .00 *
   35 -   44        .00 *
   45 -   54        .00 *
   55 -   64       2.00 I=========*
   65 -   74       1.00 I====*
   75 -   84       3.00 I==============*
   85 -   94       9.01 I============================================*
   95 -  104       5.01 I========================*
  105 -  114       8.01 I=======================================*
  115 -  124       4.00 I===================*
  125 -  134       7.01 I==================================*
  135 -  144       7.01 I==================================*
  145 -  154       2.00 I=========*
  155 -  164       5.01 I========================*
  165 -  174       2.00 I=========*
  175 -  184       3.01 I==============*
  185 -  194       3.00 I==============*
  195 -  204        .00 *
  205 -  214       1.00 I====*
  215 -  224       1.00 I====*
  225 -  234       1.00 I====*
  235 -  244       1.00 I====*
  245 -  254        .00 *
  255 -  264       2.00 I=========*
  265 -  274       1.00 I====*
  275 -  284       1.00 I====*
  285 -  294       1.00 I====*
-----------------------------
    5 -  294      70.08
```

As Received

CHEMICAL EXPLANATIONS

Recombination of D_2 and O_2 : No

Pd lattice yield stress 2600 atm (higher after hardening)
Pd $L_s \approx 4$ eV. Bond strength 0.67 eV.
20 W/ml for 100 k ≈ 0.75 keV/Pd atom

Formation of Pd-D alloy : 9 kcal/mole
= 0.4 eV
1.05 Wh/ml
≈ 170 seconds of anomalous heat (at 20W/ml)
Formation of Pd-Li alloy : Similar

CONCLUSIONS

- MICROCALORIMETRY
  - Excess Heat Generation Only With Pd/LiOD and Not With Pd/LiOH and Pt/LiOD
  - Substitution of LiOD With NaOD Reduces Heat Generation to Negligible Values
  - Changing Electrolytes From Natural 0.1M LiOD (94% Li, 6% ^{6}Li) to ^{7}LiOD Results in Reduced Excess Heat Generation Rates
  - With ^{6}LiOD Electrolyte, Response Time For Excess Heat Generation Rate Is Reduced

- NUCLEAR REACTION PRODUCTS
  - No ^{3}He or ^{4}He in Pd Samples (as Received and After Excess Heat Generation) - Mass Spectrometry
  - Tritium in Electrolyte After Excess Heat Generation Same Level as Background - Liquid Scintillation

- SURFACE ANALYSIS
  - Lithium on Electrode (Penetration ≈ 1μm) - SIMS
  - Fe, Cr, Mg, Ca,Si - SIMS, EDX
  - DendriteSEM

ACKNOWLEDGEMENTS

- ^{3}He and ^{4}He Analysis,
  Atomics International Division
  of Rocketdyne Corp. and
  Lawrence Livermore Laboratory
- SIMS - Lawrence Livermore Laboratory
- ^{6}Li Supply - Oak Ridge National Laboratory
- EPRI Sponsorship is Gratefully Acknowledged

Run 10 $P_i = (E-1.48)\cdot I$.

5/26 0.5 mm dia. Pd. [annealed 950°C, 1hr, 10^{-6} torr, furnace cool.. in vacuum.]

0.1M LiOH . charged at bench for 2 days.
(7.8 ml)

day	time	i (mA)	V	IL (mW)	out (mW)	gain	W/cc
5/26	12:15 PM	9.585	2.50	9.2	—		
	3:30 PM	9.600	2.60	10.17	18.12		
	5:00 PM	9.627	2.60	10.20	18.00		
5/27	~~5:00 P~~ 9:30 AM	9.746	2.73	11.89	18.00		
	9:40 AM	96.345	~~[illegible]~~				
	11:30 AM	96.400	4.325	274.40	271.8	~~3.3~~	
	1:00 PM	96.35	4.335	275.08	271.8		
	6:00 PM	96.40	4.330	274.74	271.8		
5/28	10:00 PM	96.55	4.295	271.79	271.8		
	11:30 AM	96.60	4.295	271.93	271.80		
	8:00 PM	96.61	4.280	270.51	268.18		
5/29	8:00 AM	96.60	4.232	265.84	264.55		
	8:15 AM	open calorimeter and measure solution amount electrolyte was : 6.1 ml ∴ 1.7 ml / 2 days. loss						
	12:00 AM	96.58	4.572	294.47	293.54		
	3:00 PM	96.56	4.525	293.83	291.73		
	5:00 PM	96.45	4.520	293.20	291.73		
	10:00 PM	96.60	4.50	291.73	289.92		

< Continued from page 28 >

day	time	i (mA)	V	IL	out	gain
5/30	8:10	96.61	4.45	286.90	286.30	
	11:00 AM	96.58	4.43	284.91	284.48	
	3:30 AM	96.55	4.41	282.87	282.67	
5/31	8:00 AM	96.60	4.33	273.88	273.62	
	3:30 PM	96.58	4.32	274.29	273.62	

* take out cell and stop { $\boxed{\text{pH}}$ before : 13.10
after : 13.25

analysis of electrolyte by nuclear engineering

no Tritium {
(A) regular 0.1 M LiOD
(B) used for charging at 60 mA/cm^2 for 31 days with 2 mm diameter sphere Pd.
(C) used for (B) + 20 hour at 600 mA/cm^2

Run 11

5/31 { 0.5 mm diameter, 1 cm annealed and furnece cooling in vacuum
(annealing : 950°C 1 hr, 10^{-5} torr)
used sample : See page 21.

Volume of Pd : 1.9642×10^{-3} cc

day	time	i (mA)	V	IL	out (mW)	gain
5/31	4:00 PM	9.847	2.56	$\boxed{\text{* 0.1 M } {}^{6}\text{LiOD}}$ (7.8 ml).		
	5:00 PM	9.794	2.611	10.49	10.87	
	8:30 PM	9.816	2.400	8.64	7.07	
6/1	8:30 AM	9.879	2.425	8.74	7.07	
	12:40 PM	9.792	2.421	8.62	6.52	
	2:45 PM	9.789	2.422	8.63	6.91	
6/2	8:10 AM	9.86	2.43	8.77	7.25	
	* change solution to new 0.1 M ⁶LiOD (7.9 ml).					
	11:00 AM	9.819	3.07	15.02	12.0	
	12:00 PM	9.809	3.04	14.71	12.14	
	2:00 PM	9.800	3.00	14.30	12.14	
	4:30 PM	9.800	2.95	13.82	12.14	
	1:30 PM	9.819	3.262	16.90	16.85	
	7:00 PM	9.795	3.265	16.89	16.85	
	10:15 PM	9.832	3.2682	16.93	16.85	
6/4	8:30 AM	9.849	3.2734	17.14	17.21	

9:40 AM increase current to 600 mA/cm²

[Continued from page 30]

day	time	i(mA)	V	IL (mW)	out (mW)	gain (mW)	W/G
6/4	9:40 AM	96.90	5.36				
	4:30 PM	97.00	5.07	342.41	349.716	7.30	3.92
	7:5 PM	96.90	5.04	339.15	360.58	21.44	10.92
6/5	7:35 AM	96.90	4.98	333.33	357.87	24.54	12.50
	9:00 AM	96.90	4.97	332.37	"		"
	12:10 PM	96.91	4.94	329.49	358.77	29.29	14.92
	1:50 PM	96.92	4.94	"	"	"	"
	5:00 PM	96.75	4.92	327.01	355.15	28.14	14.39
	9:15 PM	96.90	4.89	324.61	353.34	28.73	14.64
6/6	7:45 AM	96.95	4.83	318.96	344.28	25.32	12.66

8:40 AM { change solution to 0.1M NaOD (7.8 ml)
collect used solution (0.1M ^{6}LiOD) ⇒ see page 26. (#2)
⇒ solution level ≑ 6.6 ml   ΔV = 1.3 ml loss/2day

	9:00 AM	96.92	5.39				
	10:15 AM	96.92	5.34	368.29			
	1:00 PM	96.92	5.29	363.45	371.46	8.01	4.00
	3:00 PM	96.90	5.27	361.43	369.65	8.22	4.11
	5:20 PM	96.80	5.26	360.01	366.02	6.01	3.00
	7:15 PM	96.74	5.26	359.87	364.21	4.34	2.17
6/7	7:30 AM	96.85	5.22	356.78	362.40	5.62	2.81

10:30 AM remove cell (electrolyte ~7.0 ml ∴ ΔL = 0.8 ml loss/day)

purity : { ^{6}Li : 98.68 % from ONR.
{ ^{7}Li : 99.9 + % from Eagle/Picher.

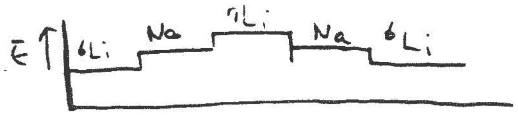
[continued from page 31]

day	time	i (mA)	V.	IL (mW)	out (mW)	gain	Power/Vol w/cc
617	change cell (Teflon coated nickel cell) electrolyte : 0.1 M NaOD (7.9 ml)						
	11:00 AM	96.72	5.71	403.28			
	12:40 PM	96.83	5.70	402.81	407.7	4.89	2.49
	2:45 PM	96.81	5.68	401.76	405.89	4.13	2.11
	4:10 PM	96.83	5.68	400.87	405.89	5.02	2.56
	5:35 PM	96.87	5.67	400.07	405.89	5.82	2.97
	9:00 PM	96.90	5.65	398.26	404.07	5.81	2.96
618	8:15 AM	96.95	5.60	393.62	398.64	5.02	2.56
	10:35 AM.	96.98	5.58	391.80	396.83	5.03	2.57
	[∘ change solution to 0.1 M ^{7}LiOD (7.9 ml) ∘ collect used solution (0.1 M NaOD (see page 26 #3) loss of solution = 0.8 ml / day.						
	11:10 AM	97.00	5.84	—	—	—	<0.1M ^{7}LiOD>
	1:45 PM	96.92	5.79	411.91	407.70		
	2:00 PM rearrange cell.						
	3:10 PM	96.91	5.85	417.68	422.19	4.51	2.30
	5:00 PM	96.84	5.82	414.30	418.57	4.27	2.18
	9:30 PM	96.95	5.76	409.13	413.14	4.01	2.05
619	8:10 AM	97.00	5.70	403.52	407.70	4.18	2.13
	10:00 AM	97.00	5.70	403.52	407.70	4.18	2.13

33

[continued from page 32]

day	time	i (mA)	U	IL (mW)	out (mW)	gain mW	Power/ $\frac{Pd}{Vol}$ W/cc
6/9	11:15 AM	97.00	5.69	402.55	407.70	5.15	2.63
	2:00 PM	96.90	5.68	401.16	405.88	4.72	2.41
	4:25 PM	96.85	5.65	398.05	402.26	4.21	2.15
	6:00 PM	96.72	5.64	396.55	400.45	3.90	1.99
	10:50 PM	96.95	5.62	395.55	400.45	4.90	2.50
	11:00 PM	Change cell to uncoated stainless steel cell containing the same amount of 0.1 M ^{7}LiOD used until 10:50 PM, 6/9 in Teflon coated Ni cell. amount of 0.1 M ^{7}LiOD = 6.6 ml ∴ 1,3 ml loss / 36 hours ⇒ 1.7 ml / 2 day.						
	11:05 PM	96.98	5.70				
6/10	8:45 AM	97.00	5.57	390.91	398.64	7.73	3.94
	11:10 AM	97.00	5.55	388.97	393.20	4.23	2.13
	1:30 PM	97.00	5.54	388.0	393.20	5.20	2.65
	3:05 PM	97.00	5.53	387.03	391.39	4.36	2.22
	3:15 PM	• change solution to 0.1 M NaOD (7.9 ml) • collect used solution 0.1 M ^{7}LiOD (#4, see pag 26) • using Teflon coated Ni cell.						
	3:20 PM	97.00	5.73				
	8:00 PM	96.92	5.53	386.71	389.58	2.87	1.46



Continue from page 33

Pd Vol cc	day	time	i(mA)	V	IN(mW)	out(mW)	(mW) gain	W/cc
63	6/11	10:00 AM	96.95	5.42	376.16	380.52	4.36	2.22
41		12:10 AM	97.00	5.41	375.39	380.52	5.13	2.61
.15		5:45 PM	96.97	5.37	371.39	371.46	0.07	–
49		9:00 PM	96.95	5.35	369.38	371.46	2.08	1.06
.50	6/12	8:10 AM	97.00	5.30	364.72	362.40		
		9:35 AM	• Change solution to 0.1M ^{6}LiOD (7.9 ml) • collect used solution, 0.1M NaOD (see page 26, #5) solution loss 1.7 ml/2 day • using Teflon coated cell (Ni)					
		9:45 AM	97.00	5.09	344.35			
		10:45 AM	97.00	5.10	345.32	353.34	8.02	4.09
94		12:00 PM	97.00	5.13	348.23	355.15	6.92	3.53
13		2:10 PM	96.80	5.11	345.57	353.34	7.77	3.96
55		4:40 PM	96.85	5.10	344.78	353.34	8.56	4.36
.2		7:30 PM	96.80	5.08	342.67	351.53	8.86	4.51
		10:00 PM	97.00	5.07	342.41	351.53	9.12	4.65
26)	6/13	8:00 AM	97.00	5.03	338.53	347.90	9.37	4.77
		10:15 AM	97.00	5.03	338.53	347.90	9.37	4.77
		12:30 PM	96.90	5.01	331.24	341.09	9.85	5.02

↓

~~6/13 3:30pM 96.86 (mA) 4.9 (V) 325.45 (mW)~~

day	time	i (mA)	V	IN (mW)	out (mW)	gain (mW)	W/cc
6/13	3:30pM	96.86	5.00	335.13	346.09	10.96	5.58
	5:30pM	96.80	4.99	333.96	344.28	10.32	5.26
	9:70pM	96.80	4.96	331.05	342.47	11.42	5.82
6/14	8:10 AM	96.95	4.94	329.63	340.65	11.62	5.62
	11:05 AM	97.00	4.92	327.86	338.84	10.98	5.60
	12:30 PM	97.00	4.91	326.89	337.03	10.14	5.17

12:45PM
- current off.
- Cell voltage immediately drops to 0.55V and
- steady-state value at ~ 0.46V
- no heat generation.
- collect used solution (# 6. 0.1M ^{6}LiOD, see page 26)
- solution level 6.1 ml after 52hours (1.7 ml/2 days)

- water vapor pressure at 25°C = 23.756 torr (CRC Handbook D-190)

(If P_{atm} = 750 mmHg P_{vap} = 24 torr
if V = 30 ml

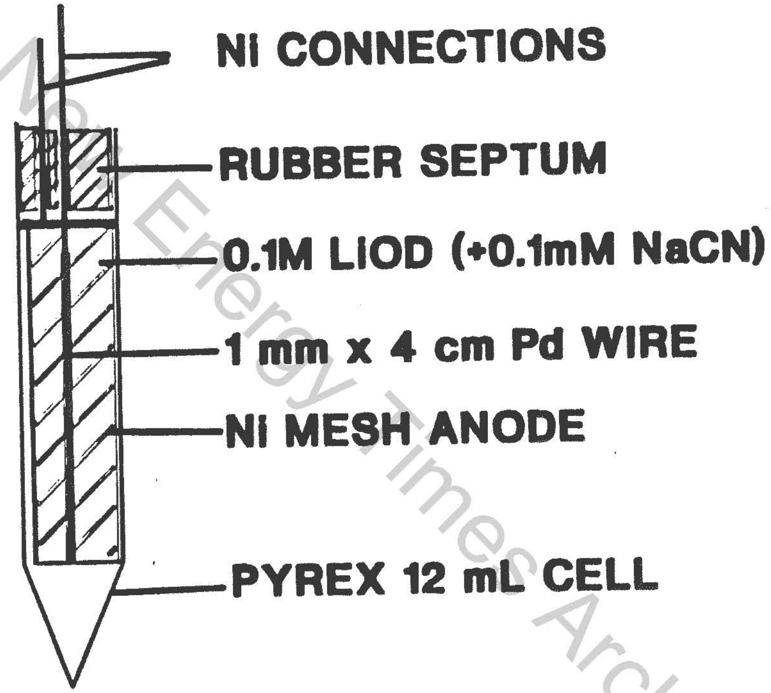
∴ {P_{gas} = (750 − 24) = 726 torr
$= \frac{726}{760} = 0.955$ atm.

$$n = \frac{PV}{RT} = \frac{0.955 \cancel{atm} \times 0.03 \cancel{l}}{0.082 \frac{\cancel{atm}.\cancel{l}}{\cancel{K}.mol} \times 298 (\cancel{K})}$$

$= 1.1728 \times 10^{-3}$ mole $= 1.17$ mM. ∴ (for $D_2 + O_2$)

if V = 8 ml ; n = 0.0003126 M ($D_2 + O_2$)

CELL DESIGN AND TREATMENT



ALL CELLS RUN AT 50 $mAcm^{-2}$ FOR 14 TO 16 DAYS

THEN AT 500 $mAcm^{-2}$ FOR UP TO 12 HOURS

2 ml/DAY D_2O ADDED AT LOW i, 5 ml/HOUR AT HIGH i

TRITIUM ACTIVITY ANALYSIS

ANALYSIS PERFORMED BY LIQUID SCINTILLATION (LSC)

INSTRUMENT USED, LKB WALLAC MODEL 1219 LSC

BIOSAFE II SCINTILLATION COCKTAIL

1 ml SAMPLE + 15 ml COCKTAIL

30 MINUTES UNDER COVER BEFORE ANALYSIS

RECOUNTS 24 HRS LATER SHOW NO CHANGE

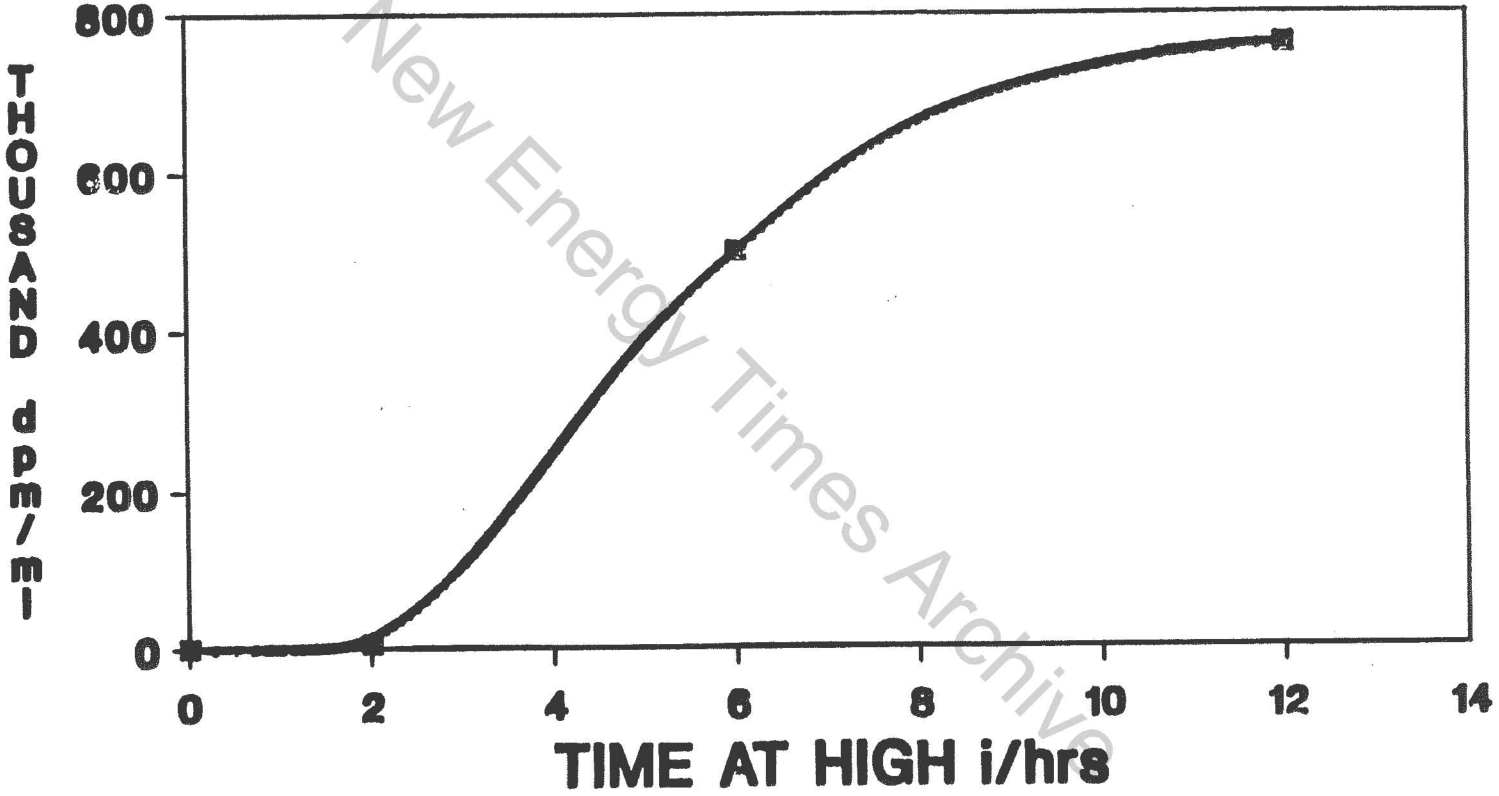
SAMPLES RUN IN BLIND FASHION

COUNTS CONFIRMED BY K. WOLF AND OTHERS

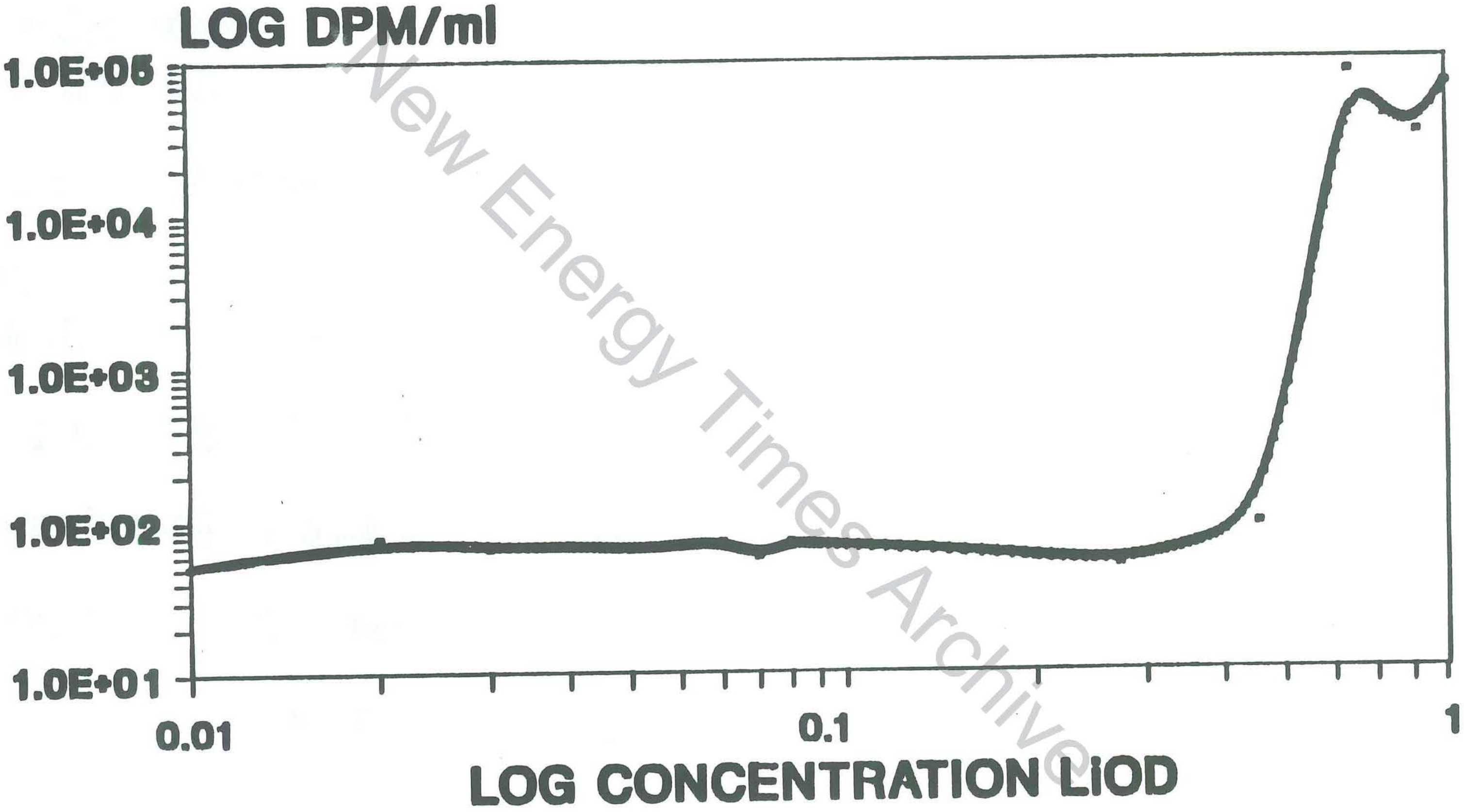
ELECTRODE PREPARATION, SOLUTION CONTENT AND ^{3}H ACTIVITY

CELL	ELECTRODE	SOLUTION	ACTIVITY (d/min/ml)
A1	NONE	0.1M LIOD	3.8E+4
A2	NONE	+0.1mM NaCN	315
A3	ANNEAL	0.1M LIOD	4.9E+6
A4 *	ANNEAL	+0.1mM NaCN	1.2E+5
A5	ETCH	0.1M LIOD	3.7E+6
A6	ETCH	+0.1mM NaCN	3.3E+4
A7	E-CHEM	0.1M LIOD	249
A7	AT 2 hrs HIGH i		5370
A7	AT 6 hrs HIGH i		5.0E+5
A7	AT 12 hrs HIGH i		7.6E+5
A8	E-CHEM	+0.1mM NaCN	339
B3 (3mm)	ANNEAL	0.1M LIOD	6.3E+4
B5 (3mm)	ETCH	0.1M LIOD	195
CELL 1 (6mm)	NONE	0.1M LIOD	264
D_2O			195
0.1M LIOD			225
NEUTRALIZED 0.1M LIOD			220
NEUTRALIZED 0.1M LIOD + 0.1mM NaCN			230

TIME PROFILE OF TRITIUM PRODUCTION FROM CELL A7



ELECTROLYTE CONCENTRATION EFFECTS ON CHEMILUMINESENCE OF COCKTAIL



TRITIUM CONFIRMATION

	CELL A5	CELL A1	STANDARD	LiOD	D_2O
TAMU	2.13E6	1304	7.23E5	240	153
ARGONNE	1.96E6	1020	7.59E5	90	114
BATTELLE	1.96E6	1170	8.08E5	127	140
GENERAL MOTORS	1.80E6	1108	-------	---	---
LOS ALAMOS		VERBAL			
LAWRENCE LIVERMORE		UNAVAILABLE			

Will Happer's comments on his visit to
Texas A&M University
with the ERAB Cold Fusion Panel
on June 19, 1989

Inspection of the Bockris laboratory. I was in the first group to visit Bockris's cold fusion laboratory. We were shown two experiments, the experiments on tritium production and the experiments on excess heat production.

The tritium-producing cells. There were close to 20 cells which were said to produce tritium. The cells were on the order of 20 ml in volume and were similar to large centrifuge cells. The cells were closed at the top with a rubber membrane, similar to the type used to cover vials of antibiotics for loading hypodermic syringes. Heavy water was added to the cells with a hypodermic syringe which was poked through the membrane before injection of the heavy water. The syringes were said to be used only once and then discarded. The rubber membrane was penetrated by the two wires used to supply the current for electrolysis and by a hypodermic needle attached to transparent plastic tubing, about 8 mm in diameter. The plastic tubing led to a beaker containing mineral oil through which the gaseous mixture of deuterium and oxygen together with heavy-water vapor bubbled up to the surrounding atmosphere at a rate of a few ml per minute. The escaping gases carried with them some heavy water and black oxides of nickel or other dirt which were entrained in the escaping gas stream because of the frothing of the electrolyte. A good deal of electrolyte and nickel oxide or other dirt had accumulated in the beaker beneath the mineral oil. I would guess that the beaker contained a few hundred ml of dirty electrolyte and a few hundred ml of oil. John Schiffer pointed out that there were two bottles of heavy water from different suppliers on the table next to the hood that contained the cells. We were assured that the origin of the heavy water was well known and recorded for each addition. None of the cells was being monitored for heat release, and most were operating in the air although several, which were operating with higher currents, were in a water bath to provide more effective cooling.

Most of the cells contained nickel anodes, which Bockris says are essential for the production of tritium. The cells had been operating for various periods of time, from days to weeks. Cells which had been operating for more than a few days were filled with a black suspension which was said to be oxides of nickel.

If I understood the cell construction correctly, there was a central wire of palladium, perhaps 2 mm in diameter, which served as the

cathode. We looked at a sample of the palladium, which was from a supplier in Virginia. The anode was a nickel mesh wrapped up into a cylindrical shape and held away from the palladium cathode by red rubber spacers which could be seen at the top and bottom of the electrode assembly. I don't know about the chemical stability of rubber in the highly alkaline electrolyte of these experiments, but I would be surprised if some of the rubber and glass was not leached away during the course of the experiments. Goodness knows what was in the solutions which were added to the tritium-counting cocktail, but it seems quite likely that there were transition metal oxides and various organic decomposition products from the rubber spacers which might have been involved in slow chemiluminescent reactions.

Access to the tritium production cells. There appeared to be no control of access to the any of the Bockris experiments. They were located in a large laboratory which contained desks for many graduate students, many of whom were working away during our visit. I don't think that there is any way to be sure that the tritium producing cells were not "spiked" by someone.

Neutron activation. If tritium were really produced by d-d fusion reactions, a comparable number of neutrons would have been released if the usual branching ratios for the fusion reaction hold. If so, neutron activation products should be readily detectable in the surrounding pieces of apparatus. An example is neutron capture by ^{23}Na to yield ^{24}Na which decays with very characteristic gamma rays and is often used to determine the dose to humans who have inadvertently been exposed to neutrons. Most glass, including pyrex glass from which the cells were probably made, contains several per cent sodium oxide as a flux. Sodium activation produces a rather short-lived diagnostic gamma ray. I do not have a table of isotopes and neutron cross sections available to me so I cannot give examples, but I suspect that one could find longer-lived neutron activation products which could serve as a retroactive test for neutrons released from the tritium-generating cells. For example, there may be convenient neutron activation products from the the nickel and palladium electrodes.

The heat-producing cells. We also inspected two cells which were said to sporadically generate excess heat. These cells were much bigger than the cells used for tritium production, perhaps 5 cm in diameter and containing around 100 ml of solution. They had a styrofoam plug at the top, and this was penetrated by a pair of leads for the electrodes and a pair of leads for the calibrating heater, which was a commercial cartridge heater in a glass tube to keep it dry. The cells were big enough to contain a plastic-coated iron stirrer on the bottom, which was spinning away while being driven by two magnetic

stirring devices located below the water bath. The plug was also penetrated by commercial thermistor probes. There was a single thermistor in one cell, and there were two thermistors in the other. The electrolytic cell was placed inside a larger-diameter cell which was immersed in the thermostatic bath. The spacing between the outer jacket and the cell was on the order of 8 mm, and the electrolytic cell was loosely held in position by two rings of plastic tubing, about 3 cm apart, near the top and bottom of the electrolytic cell. Except for the plastic spacer, the volume between the electrolytic cell and the jacket was filled with air. The thermostatic bath was filled with water at about room temperature and covered with small ping-pong balls to reduce evaporation.

Thermal coupling to air. It was not clear whether the thermal coupling to the laboratory air was negligible compared to the coupling to the bath. A large area of the electrolytic cell was directly exposed to the air, and when I put my finger on the plastic plug, I noticed that it and the surrounding glass at the top of the cell were noticeably warm. I have no doubt that significant convection currents are carrying warm air out of the top of the cells, and it is not clear how much heat is flowing into the room air and how much is flowing into the bath. More importantly, however, I was very surprised to find that the cell moved freely when I touched it, about which more below.

Loose fittings. The electrolytic cells fit very loosely in the jacket, and very minor fiddling with the wires was enough to seriously tilt the cell from one side of the jacket to the other. I estimate that the motion was typically about 5 mm. We asked one of the people who normally work on the experiment to show us how he added heavy water to the cell. He used a large and frightening hypodermic syringe, and it was necessary for him to remove a rubber stopper from the top of the cell to allow him to insert the needle of the syringe. In the process, Norman Ramsey noticed that there was about a 10% change in the reading of the thermistor on the chart paper. I had earlier noticed that by shaking the wires it was possible to change the chart paper readings by around 10% in either direction, since everything was so loosely fitting. Once made, a change in the calibration was constant until someone jiggled the wires again. While inspecting the laboratory notebooks, we saw examples of both "excess heat" and less-talked-about "deficit heat," both on the order of 10% of the nominal values. Someone, Allen Bard I believe, pointed out that the "excess heat" production reported by the Bockris group can not be distinguished from the effects of changes in the calibration of the calorimeter due to jiggling of the cell within the jacket.

Visit to the Appleby-Srinivasan laboratory. I was in the second group to visit the Appleby-Srinivasan laboratory. Their work is based on

studies of electrolytic cells in a commercial microcalorimeter. They claim to observe sporadic generation of excess heat. They have never seen any tritium production, although I gather that they have looked for it. The data that they present in viewgraphs looks "raw" but in fact it is heavily massaged and supplied with plausible looking "noise." We were permitted to inspect some of the chart paper from which the viewgraphs were made, and my judgement is that the "excess heat" is in the noise. Barry Miller, who spent more time than I did looking at the charts says that the "excess heat" generation is inferred from changes in the cell voltage. There is almost no change in the heat output.

Conducting cell walls. A serious problem with the earlier experiments was that the walls of the cell were stainless steel. Although Appleby claims that there is no net current to the stainless steel cell jacket, there is no way to estimate how much the currents carried through the conducting walls of the cell and indeed through the rest of the calorimeter, which is massive (several pounds) of aluminum blocks pressing up against the stainless steel sample cell. Cells with insulating teflon coatings are now being prepared, but I was not sure whether any had been used yet, and if so whether any of the gave excess heat.

Black-box calorimeter. It was very difficult to know what was going on in the microcalorimeter. The device was originally built to measure the heat loss attending the self discharge of commercial batteries, and it needed some modification to permit it to be loaded with electrolytic cells. Appleby et. al. do not know how the calorimeter works, but they are in the process of trying to get more information about it from the original builder. For example, they do not know where the calibrating resistors are located, they do not know how the thermopiles are arranged in the instrument or whether the thermopile voltage or current is measured. They think it is the voltage, but it it not clear from any documentation. The samples are also very well hidden inside many layers of insulation once the calorimeter is loaded, so no one can know what is going on in the cells. You can't just look inside as you can with the Bockris cells.

What did we learn?

1. The Bockris calorimeter is too loosely put together to be trusted. I don't think any excess heat was observed.
2. The Bockris tritium results are a mystery.
3. None of Bockris's colleagues has seen tritium.
4. There is no evidence for the neutrons which should have been produced along with the tritium. Someone should look for neutron activation products.

5. The controls for the tritium experiment are very poor. Many people have access to the experiment, and no one seems to be in charge. Bockris told us himself that he had many people participate in the experiments, so who knows what happened? There is a handwritten poster with instructions on how to record data over the hood in which the tritium experiment is done, presumably for the benefit of part-time helpers in the wee hours of the night. Having participated myself in a few practical jokes on senior professors (who deserved it) when I was a graduate student and post doc, I have no confidence that the experiment is secure from mischief.

6. The samples drawn out of the tritium-producing cells probably contain organic decomposition products of the rubber stopper as well as various transition metal compounds from the nickel electrode. What does this imply for chemiluminescence in the scintillator cocktail?

7. The Appleby-Srinivasan experiments are based on a poorly understood calorimeter and on signals which are very close to the noise. The stainless steel cell walls can carry current and may have shorted out, for example by frothing, some of the cell resistance, thereby making the power input to the electrolysis seem less. Since it is not possible to look inside the microcalorimeter no one could check.

23 JUN 89 13.83

-R.L. GARWIN-

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WASHINGTON, DC 20585

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COLLEGE STATION, TEXAS 77843-3255

June 22, 1989

(409) 845-2011
FAX (409) 845-4719

Professor Norman Ramsey
Harvard University
Department of Physics
Cambridge, MA 02138

Dear Professor Ramsey,

I am glad to tell you that David Worledge of EPRI called me this morning and told me that the Cold Fusion work at Los Alamos has led to the confirmation of tritium production from D_2O (100xbackground); and the confirmation of the production of excess heat.

I have no need to comment on this very important news.

Yours sincerely,

J. O'M. Bockris
Distinguished Professor of Chemistry

JOMB/nr

9B

DOE F 1325.7
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FROM: BILL WOODARD SECRETARY, COLD FUSION PANEL (202) 586-5767
(Name/Office Symbol/Telephone No.)

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Remarks:

Attached is the trip report by John Schiffer on the visit to Brigham Young University on June 12, 1989

3-1080 WE

Comments on the visit to Brigham Young University
with the ERAB Cold Fusion Panel
on June 12, 1989
by John Schiffer

This is to summarize my own impressions of the visit to BYU. We were received hospitably and the discussions (almost entirely with Steven Jones) were open, though he was a little vague on some points (do not mean to suggest that this was deliberate, just my impression of his knowledge of the details).

There was some talk of results at Grand Sasso and at Los Alamos, but while Jones is a collaborator on these it was also clear that he was not so knowledgable about the details and will not comment on those here -- instead discuss only the data taken at BYU on which we could get the most direct information. Nor do I comment on the composition of electrodes and electrolytes that were discussed briefly.

Protocol of measurements:

Jones was questioned on the methods by which he compares 'foreground' runs to 'background' ones. We were told that these were done under identical circumstances with and without a heavy water cell that had a current running through it. When we saw the apparatus it did not seem to be firmly anchored in place, the shielding consisted of boxes of pennies, labelled '$25", (do pennies have lower background, we never asked) but it did not seem convincing that any special care was taken to clamp the shielding in place -- small gaps in it were evident, these could change if someone changed the position of the cell or the neutron counter.

In any case, foreground and background runs are of drastically varying

4-1080 WE

lengths -- I had concluded from the error bars shown in the Nature article, that if some were 8 hours in duration (as is said in the article) then others must be for a week, and yet others on the order of one hour. Jones at first denied that there were such variations, but later he looked in his logbook and the variations were indeed of the order suspected. It is still not clear to me how they combine 'background' runs for the individual points shown in their paper; some of the things said implied that they may use the same background runs for several data points in the Nature article.

Fitting the total data:

But they have combined all their background runs and did a new fit, with a simple three-term polynomial obtaining a chi-squared of 137 with 132 degrees of freedom. They then take the coefficients defining the shape of this polynomial and fit its magnitude, with a three-parameter gaussian to their 'foreground' run. The chi-squared here was 108/131. With the gaussian established to 4.5 standard deviations (fits enclosed).

Selection of data:

Questioned asked whether there were any selection criteria for excluding runs and was told at first 'no', all with current and heavy water were included, then this was corrected to say that one of the later continuous four-day runs was not included, because by then they had discovered that the neutron effect supposedly disappeared after 8 hours. Yet the earlier long runs were retained. The total foreground running time appears to be roughly on the order of 20 days (my estimate). There is therefore a little arbitrariness in choosing what was included and what not -- it may slightly alter the quoted 4.5 sigma effect.

Cosmic ray variations:

The other major point had to do with the question of variation of the cosmic-ray neutron background. I asked whether they had considered this -- but they seemed unaware of any such effect. I pointed out that there was a well established coefficient for cosmic-ray neutron flux variations with barometric pressure, -0.71 %/mbar, that could give 10 to 20 percent fluctuations in neutron flux -- they had not considered this. The defense was that cosmic ray fluctuations would not give a well-defined Gaussian peak, but the whole area under the peak is not so large, so one cannot build too much on the shape. I have sent them some references for this [Cosmic Rays by Gorman, North Holland 1974] and also found a reference in the literature for a 550% increase in neutrons associated with a solar flare (seems to be the largest on record), that I enclose. Jones said that perhaps they would put a student on correlating their runs with barometric pressure records.

Relation to Menlove's 'bursts':

We also asked what their signal would look like if the neutrons came in sharp bursts, as Menlove's data seems to indicate. The BYU detector efficiency for a first hit is about 10 %, and for the Li tagging another 10%, 1 % overall (compared to about 30% efficiency at LANL). But there could be multiple neutron hits in the scintillator followed by one Li tagging signal and such double hits would appear as higher energy neutrons, not within their 'gaussian'.

Sources of background:

There were two small Van de Graaf accelerators in the same hall but we were told that they were used for callibration and not turned on (e.g. by students at night for a short test) during data collection. Also questioned about neutron sources in the vicinity and was told that the

6 - 1080 WE

sources (RaBe and Cf fission) were not locked up, however the students were scared to handle sources and only faculty moved them. Personally, I would have kept the sources, and the accelerators locked up for the duration of the measurement -- just to avoid some careless student turning on the terminal voltage for a 'test' or moving a source to check a detector late at night. There were many (at least half a dozen) students in evidence in the hall.

Neutron detection:

Had a conversation with Czirr -- he is a long-term neutron detector specialist, seems to be copmpetent and careful, and as far as I could judge the detector is a reasonable one.

Overall impression:

The BYU experiment, on its own, is reasonable within its limitations. The care taken in the data runs, assuring strictly identical conditions between foreground and background runs, does not seem to be as strict as one would like to see for establishing a statistically marginal effect, whose existence is likely to be widely questioned. There was a lot of loose talk of individual runs showing time dependences, and of the nature of the gaussian peak, that in my view was questionable because of the marginal statistics. Overall, if all circumstances were strictly as constant as the BYU group assumes, there seems to be a small effect. If it stands up, after being corrected for barometric variations, and if one can establish that there were no dramatic variations in solar activity, the overall effect could be real (though many of the details about time variations seem anecdotal and were not demonstrated to us quantitatively; e.g. leaving out some fraction of the data is questionable and may reduce the effect to

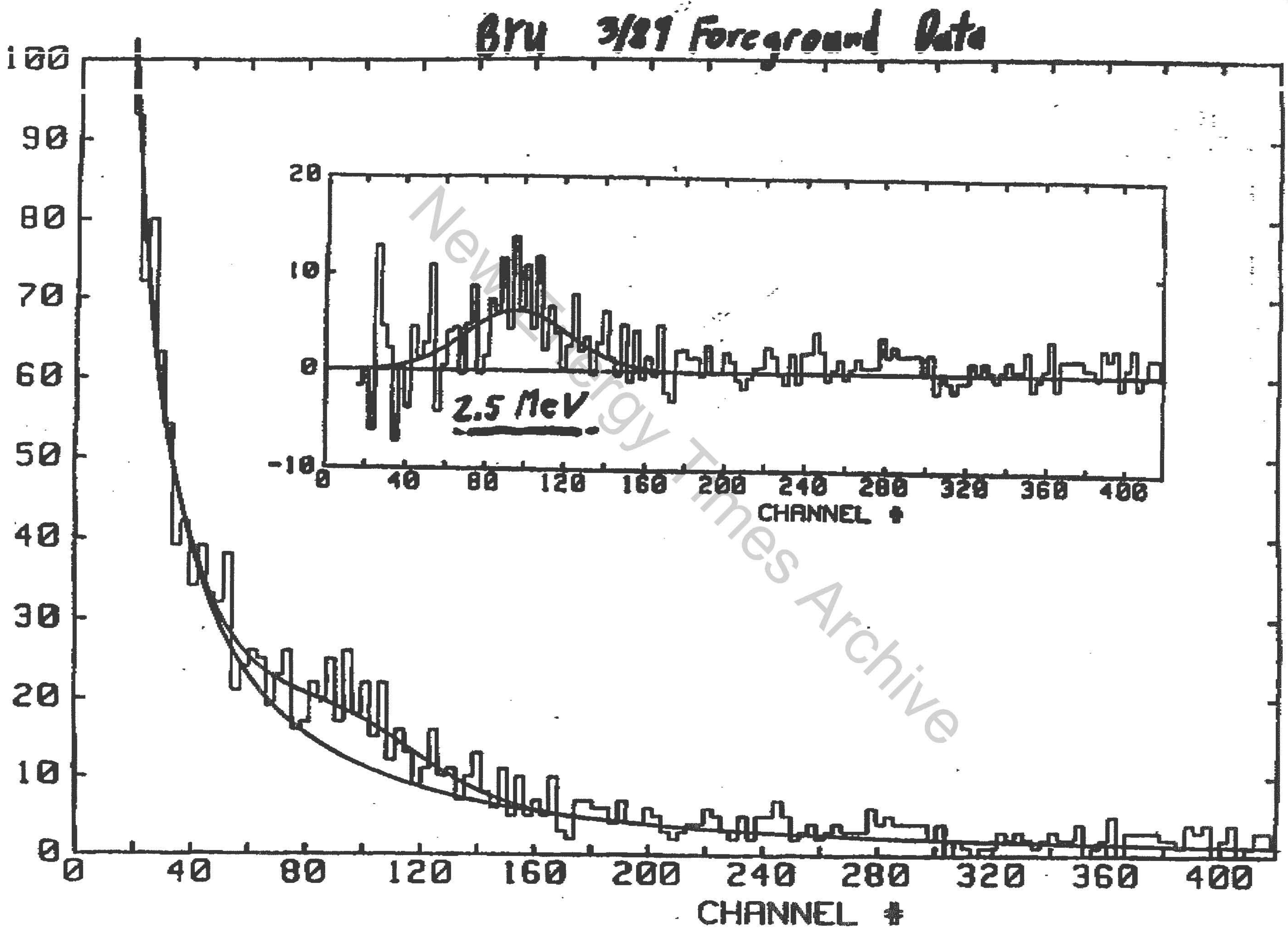
7-1080 Wz

perhaps three standard deviations). If real, the effect could be similar in origin to Menlove's data (bursts).

Enclosures:

1) new curves from Jones;
2) figure on barometric pressure dependence of cosmic ray neutron flux;
3) copy of article showing a large solar-flare related neutron burst.

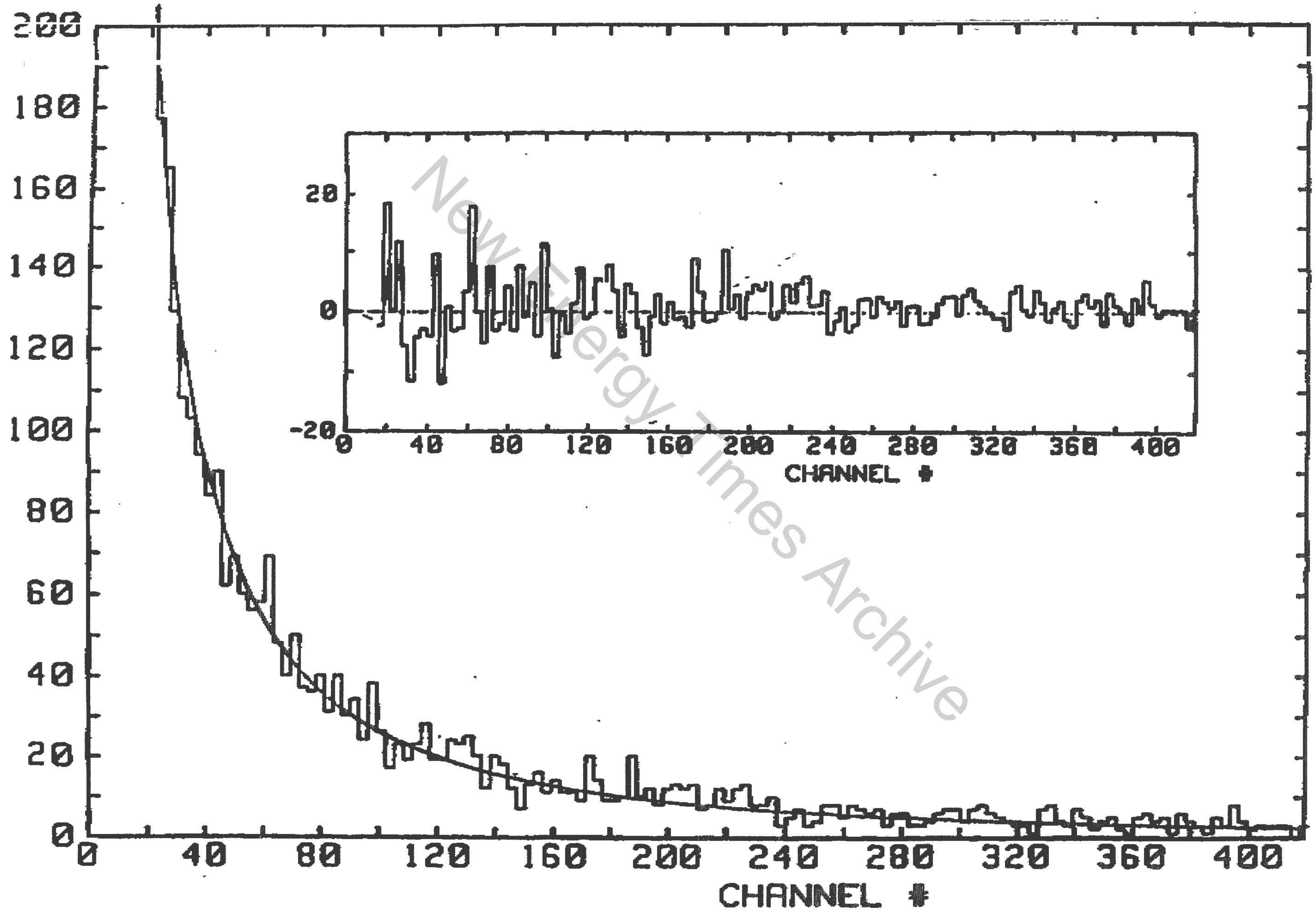
1)


BYU 3/81 Foreground Data

9-1080we

1)

Background data



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2)

Fig. 17.4. The barometric coefficients for the neutron component of cosmic rays. I–VII are results obtained by different authors with their measuring errors. A – at Mawson; B – at Mt. Wellington.

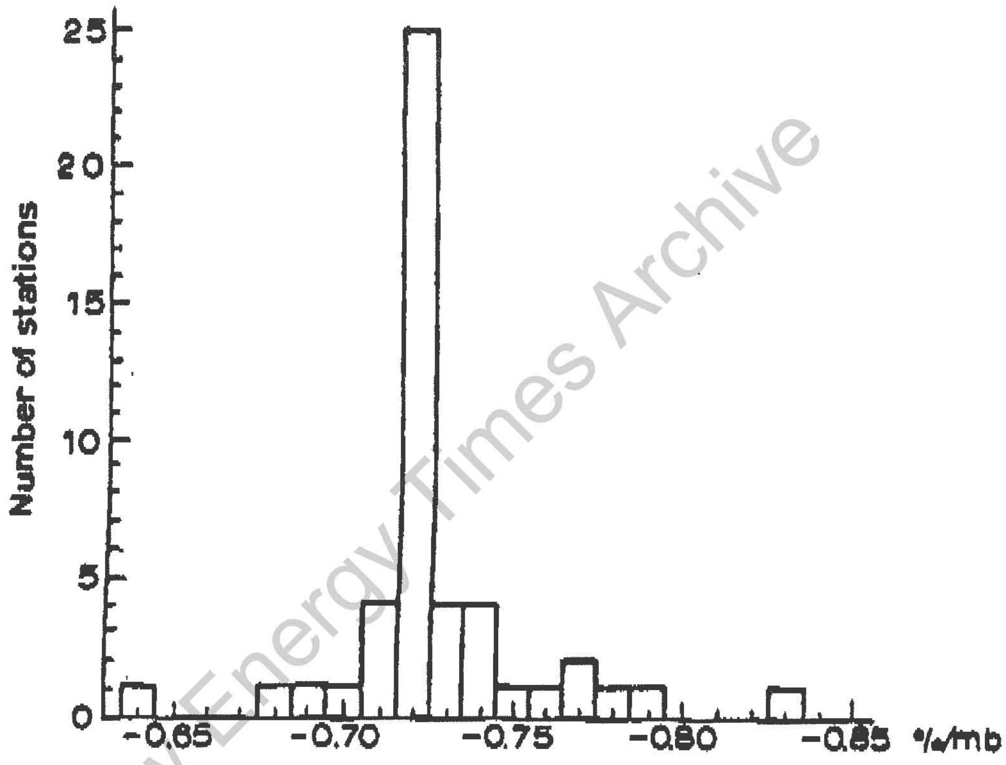


Fig. 17.5. Histogram of the barometric coefficient for the world-wide net of neutron monitors.

From L. I. Dorman
"Cosmic Rays" North Holland
(1974)

11-1080WE

~~The measured value of the K.E. release in the slow neutron fission of ^{235}U is given by Brunton and Hanna (1949) as 151·5 MeV. and from this we derive 124 ± 10 MeV. as the mean kinetic energy released in the photofission of ^{232}Th.~~

~~We wish to thank Mr. L. R. B. Metcalfe for help in setting up the experiment and for operating the synchrotron for these exposures.~~

~~Atomic Energy Research Establishment,~~
~~Harwell, Berks.~~
~~16th February, 1950.~~

~~E. W. Titterton.~~
~~T. A. Brinkley.~~

~~References.~~

~~Bohr, 1941, *Phys. Rev.*, 59, 270.~~
~~Brunton and Hanna, 1949, *Phys. Rev.*, 75, 990.~~
~~Dewan and Allen, 1949, *Phys. Rev.*, 76, 181.~~
~~Titterton and Goward, 1949, *Phys. Rev.*, 76, 142.~~
~~Tsien and Faraggi, 1947, *Compte-Rendus*, 225, 294–6.~~
~~Westöo, 1948, *Ark. Mat. As. O. Fys.*, 34 B, No. 22, 1–8.~~

---

3) Phil. Mag. 41 (1950)

A Temporary Increase in the Neutron Component of Cosmic Rays.

By N. Adams,
The Physical Laboratories, The University, Manchester.

[Received March 10, 1950.]

A recent report (Forbush, Stinchcomb and Schein 1950), has drawn attention to an abnormality in the cosmic ray intensity on November 19, 1949.

Increases of 43 per cent were found in the ionizing cosmic rays measured at sea level in geomagnetic latitudes 50° and 80°, and an increase of 180 per cent in the rays measured at Climax, Colorado, at an altitude of 11,000 feet. No increase was found at 11,000 feet on the equator. These increases were associated with a radio fade out at 10.33 G.M.T. At about the same time large increases were found in the cosmic ray intensities recorded by two independent systems in these laboratories.

One of these which records ionizing cosmic ray consists of three-fold Geiger-Muller counter telescopes pointing north and south at an angle of 45° to the horizontal (Elliot and Dolbear 1950). This recorded an increase in intensity reaching a maximum of about 12 per cent above normal between 11.00 and 12.00 hrs. Its counting rate had reverted to within 1 per cent of the normal by 14.00 hrs. The other is a neutron intensity recorder and the accompanying figure shows its response for the period 19th–20th November, 1949.

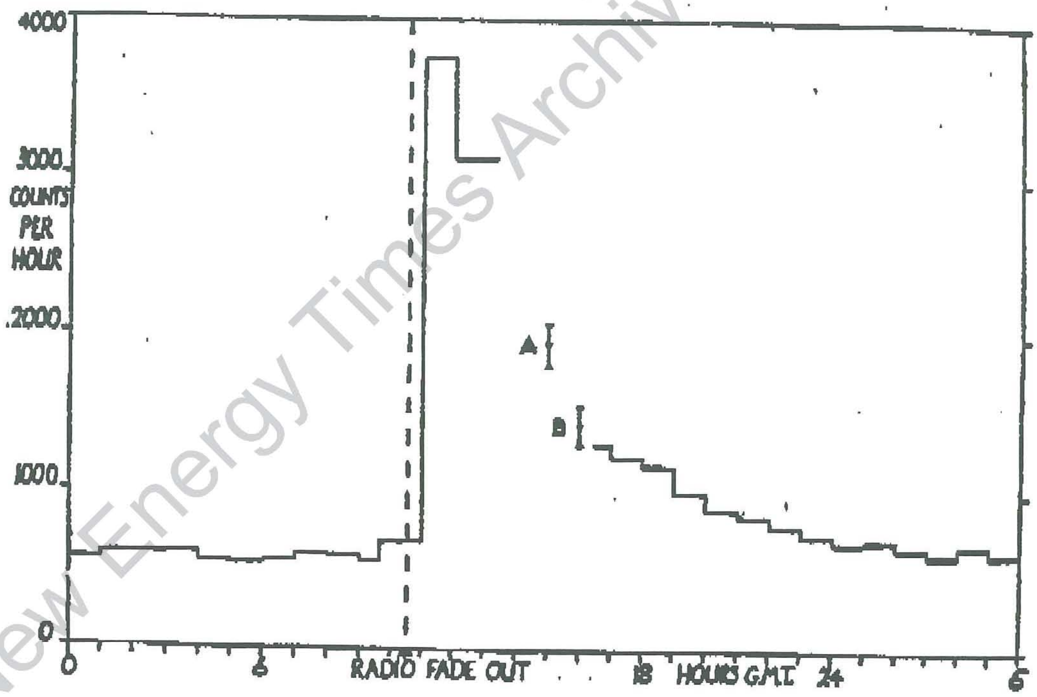
12-1080WE

3)

The neutron detector consists of boron–trifluoride filled proportional counters embedded in a "Pile" built of high purity graphite. Its detection efficiency is appreciable for incident neutrons of energies between thermal and perhaps 10 MeV., being greatest for energies 3 to 5 MeV.

The ordinates of the figure represent the observed average counting rate during successive hourly intervals. No hourly readings are available for the period between 13.25 and 16.27 hrs. when the equipment was being tested. However, counting rates taken for periods of a few minutes each and therefore of rather low precision, were obtained during this period at about 15.00 and 16.00 hrs. and are plotted in the figure with appropriate standard deviations as A and B respectively.

The barometric pressure for the period shown in the figure had extreme variations of ±0·1″ Hg about its mean value. Such variations would produce changes of about ∓2·5 per cent in the counting rate of the recorder in normal conditions.



From the moderate increase of counting rate (15 per cent) observed during the period preceding 11.00 hrs. and the enormous increase (550 per cent) in the period 11.00–12.00 hrs. (both compared with the counting rate earlier that morning) is seems reasonable to suppose that the onset of the disturbance occurred only a few minutes before 11.00 hrs.

After reaching a maximum value for the period between 11.00 and 12.00 hrs. the counting rate, so far as readings are available, decreased continually with time and was still significantly above normal in the first hours of November 20th. Thereafter the hourly counting rates showed their usual statistical fluctuation.

Except for the occasion described here no variation of counting rate of root mean square amplitude (averaged over periods of several weeks) greater than 0·3 per cent of the total rate has been found save for the effect of barometic pressure.

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